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THE LAW OF ATTRACTION IN RELATION TO SOME VISUAL AND TACTUAL ILLUSIONS.

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In working over the results of some experiments which were published under the title, 'Ueber den Einfluss von Nebenreizen auf die Raumwahrnehmung,'[1] in which was demonstrated a tactual illusion similar in nature to the Müller-Lyer visual illusion, I observed that the influence of a secondary stimulus (Nebenreiz) in producing an elongation of a primary linear stimulus was directly proportional to the intensity of the secondary stimulus and inversely proportional to the square of the distance between the two stimuli. The number of specific instances upon which this observation was based was somewhat small and the number of variations in distance and in intensity of secondary stimuli were very limited. Moreover, data for determining the influence upon the result which might be occasioned by the variation in intensity of the primary stimulus were almost wholly lacking. On account of these and other similar deficiencies, it was not thought wise at the time of the former publication to propose a hypothesis of such apparently far reaching significance.

Starting, however, with this hypothesis in mind I have directed numerous other experiments, designed to reveal the exact relation between primary and secondary stimuli and the effect in perception of the one upon the other. Inasmuch as it was so clearly shown[2] that the tactual illusion was similar in

[1] *Archiv fur die gesamte Psychologie*, Vol. I., pp. 31-109.
[2] *Ibid.*

almost every detail to the visual illusion, I selected the latter for the investigation. The visual figure lends itself more readily to small and numerous variations, the mean variation in any series of judgments is much smaller and it seems to me that the operation of a law such as that indicated would be more easily detected because more uniform in its manifestations.

The present paper, therefore, is a report of some experiments made for the purpose of determining quantitatively the influence exerted by secondary visual linear stimuli upon a primary visual linear stimulus, or perhaps more accurately stated, the attempt is to determine in mathematical terms the attractive force operative between two visual stimuli. It will be understood, of course, that the limitations of language confine us to the use of the term 'stimulus.' If there be, in truth, any attractive force operating between the actual objective stimuli, it is certainly not our present purpose to determine that. Stimulus, as I have used the term, represents an 'impression' made upon a sense organ. It is not yet, necessarily, sensation; it is certainly no longer stimulus. It is rather a middle state, viz., a state of the nervous system occasioned by objective stimulus and the conditioning element in sensation.

I began, first, a series of experiments with a figure of the Müller-Lyer type. The projecting arms were turned outward, and separated from the central line, or primary stimulus, by small open spaces as represented in Fig. 1 (Plate I.).

In general, the results were of the sort which I had expected, but it soon became manifest that another factor in addition to distance and sensation intensity was playing a part in the results. It is a well-known fact that the Müller-Lyer illusion varies with the cosine of the angle formed by the projecting arm and the central line.[1]

In order to eliminate this third variable factor, I abandoned the Müller-Lyer figure entirely and constructed a figure with one central linear stimulus and two other simple linear stimuli, which I have termed the secondary stimuli. The secondary stimuli were constructed exactly in the line of direction of the

[1] Heymans, *Quantitative Untersuchungen ueber das optische Paradox* in *Zeitschrift f. Psychol.*, Vol. IX., p. 221.

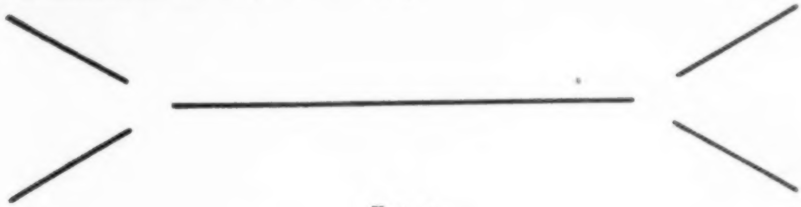


FIG. 1.

FIG. 2.

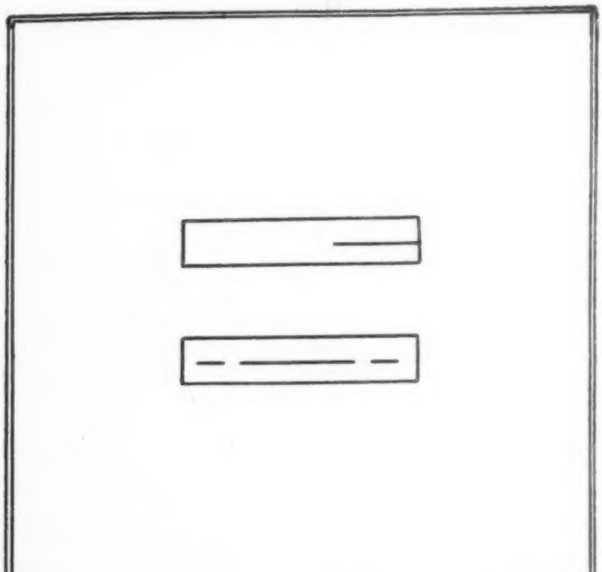


FIG. 3.

central, or primary, stimulus but separated from it by small open spaces.

The accompanying Fig. 2 makes clear this construction. AB is the primary stimulus, EC and DF are the secondary stimuli. The distance between primary and secondary stimuli is measured of course from center to center and is xy in the figure.

It will be observed that several radical variations of this figure may be made. We may vary (1) the distance xy, or (2) the stimulus AB, or (3) the stimuli EC and DF, retaining in each case all other factors constant. The results of our experiments, therefore, fall naturally into three groups: (1) The effect of secondary stimuli at different distances, (2) the effect of secondary stimuli when the primary stimulus is varied, (3) the effect of secondary stimuli of different intensities upon primary stimuli of a constant intensity and at the same relative distances.

Method of the Experiment.

Preliminary experiments were conducted in order to determine the best method. (1) A card upon which was drawn the figure to be judged was given the subject and, in addition, a series of cards containing each a single line but of different lengths. The subject was required to select from the series of cards the one containing the line apparently equal in length to the primary stimulus in the figure. (2) Instead of a series of cards, a single card upon which was drawn a series of lines of different lengths was used. The subject was required to designate in the series of lines the one which appeared equal to the primary stimulus. (3) Instead of a series of lines, a single long line was drawn upon a piece of cardboard, and the cardboard was adjusted to slide back and forth through a slit cut in another piece of cardboard. By this means it was possible for the subject to make the line longer or shorter until it seemed to him equal in length to the primary stimulus.

The method finally adopted and which I think will be recognized as the most convenient and accurate of the four methods which were tried, was as follows: A frame, three feet square, was hung upon two upright posts which projected three feet

above a low table. In this frame was fixed a square of cardboard containing near the center two rectangular openings. On the rear side of the frame and parallel to the openings were tacked wooden runners or grooves, so adjusted that one could slide certain cards, containing the figures to be judged, into their proper positions filling the rectangular openings just referred to. When in position and ready for the experiment, the frame appeared to the subject as shown in Fig. 3. The upper single line could be lengthened or shortened by sliding the card back and forth. On the back of this card was a millimeter scale, so arranged that the experimenter could read immediately the length of the line as it appeared to the observer. The experimenter, seated behind the screen at the table, could move the card easily back and forth and record immediately the reading of the millimeter scale, which registered the judgment of the observer.

The observer was seated in front of the screen at a distance of 80 cm. His task was to observe the moving upper line and the lower stationary figure at the same time and to say 'stop' as soon as the *difference* between the upper line and the lower primary stimulus *ceased to exist*. This form of instruction to the observer was adopted because it was noted that if told to say stop when the two lines appeared to be equal, the subject adopted, somewhat irregularly, either of two courses: (1) She said 'stop' when the difference *ceased*, or (2) having allowed the variable to pass the point where the difference ceased, she said 'stop' not until she began to perceive a difference in the other direction. In order to secure relative constancy, the former type of reaction was insisted on. In half the experiments constituting a series, the moving, variable line was gradually lengthened and in the other half of the series this line was gradually shortened.

Method of Estimating the Influence of the Secondary Stimulus.

It seemed natural at first thought to estimate the influence of the secondary stimuli as equal to the difference between the length of the primary stimulus as given by objective physical

measurement and the length of a second line which is judged by the subject to be of the same length.

Preliminary experiment, however, showed clearly that when a subject attempts to estimate the length of a single line (without secondary stimuli), using the method above described, the judgment is always too small, *i. e.*, the line is always judged to be shorter than it actually is. Consequently, in order that the secondary stimuli may produce subjectively an elongation of the line objectively given, the tendency to shorten just observed, must first be overcome. Inasmuch as the addition of a secondary stimulus accomplishes this, we must include this in our estimate of the influence of the secondary stimulus. Accordingly in every series of judgments of a line accompanied by secondary stimuli, I have required evenly distributed judgments of the same line without secondary stimuli. The results, therefore, which appear in the tables as 'influence of the secondary stimulus' always represent the difference measured in centimeters between the judgment of the line without secondary stimuli and the judgment of the length of the same line with secondary stimuli.[1]

The foregoing is in general the method of experimentation and computation of results employed in each of the three groups of experiments which follow. In connection with each group some further details of method must be pointed out.

[1] Professor Judd (*Genetic Psychology for Teachers*, p. 11) has attempted to explain the fact that if one tries to draw upon paper a line equal in length to a copy upon the blackboard, he invariably makes it too short, as due to the larger environment represented by the blackboard as compared with the smaller environment represented by the sheet of paper. The facts brought out in my experiments seem to throw serious doubt upon the adequacy of Professor Judd's explanation. In my experiments, the environment of the two lines judged to be equal was the same, and moreover, the error remained the same when the relative position of the two lines was reversed.

The explanation of the error is probably as follows: When I am comparing two lines, one standard and the other variable, the latter is the one which is kept most prominently in the foreground of attention. The eye wanders to the standard only to renew the memory of its length. What actually happens is a comparison of a present vivid, intense sensation with a fading memory image. I may fixate the standard but by the time my eye reaches again the line which I must make equal to the standard, the latter has become a memory image, or at best appears upon the periphery of vision and consequently has less sensation value than the same image upon the fovea centralis or in the focus of attention.

The Effect of Two Secondary Stimuli Upon a Primary Stimulus When the Distance is Varied.

I present first three tables, I., II. and III., showing results for a primary stimulus of 16.0 cm., 17.0 cm. and 18.0 cm., respectively, in length. The experiments with the three different primary stimuli, though recorded in different tables, were conducted simultaneously. For example, a card containing a primary stimulus 16.0 cm. and secondary stimuli 9.5 cm. distant was presented to the subject. Five successive judgments of this same line were required, the variable line being first lengthened and then shortened and so on alternately for the five judgments. Then a second card containing primary stimulus 17.0 cm. was presented and five successive judgments of this line in a similar order were required. Then primary stimulus 18.0 cm. was presented and the same judgments required. Now we return to primary stimulus 16.0 cm. but one in connection with which the distance of the secondary stimuli has been slightly increased, viz., 10.0 cm. Then the series 17.0 cm. and 18.0 cm. with similar increase in distance of secondary stimulus are taken, and then back again to primary stimulus 16.0 cm. with distance of secondary stimuli still further increased and so through the entire series of five variations in distance for each of the three primary stimuli. Including the three cards which contained only a single line each, to which reference has previously been made, there were eighteen different cards and five judgments of the stimulus on each were required. Such a series could be made in about a half hour, which was the length of a setting for each subject.

At the second sitting, the experiment was conducted in a similar manner, except that the detail in every particular was reversed. Two sittings afforded a series of ten judgments each for each of the eighteen primary stimuli. The tables show results for ten different subjects and each result given is the average of ten individual judgments made at two different sittings. There are two such series for each subject and the general average for the ten subjects represents in each case two hundred individual judgments.

The subjects used were of varied age and character. One is my colleague, Professor Essary, of the department of biology, to whom I am under especial obligation; another was a student in the department of psychology; a third was a special student of painting, and a fourth was a special student of music. The remaining six were taken indiscriminately from the preparatory school of Brenau College and vary in age from ten to fourteen years. All except the first mentioned are female.

TABLE I.

INFLUENCE OF TWO SECONDARY STIMULI, EACH 2.0 CM. LONG UPON A PRIMARY STIMULUS 16.0 CM. LONG AT DISTANCES 9.5, 10.0, 10.5, 11.0 AND 13.0 CM.

Distances.		9.5 cm.	10.0 cm.	10.5 cm.	11.0 cm.	13.0 cm.
Subject.	Series.	Influence of Secondary Stimulus in cm.	Influence of Secondary Stimulus in cm.	Influence of Secondary Stimulus in cm.	Influence of Secondary Stimulus in cm.	Influence of Secondary Stimulus in cm.
E.	1	1.49	1.18	1.11	.46	.04
	2	1.61	1.16	.81	.32	.08
Pa.	1	1.17	.81	.72	.18	—.01
	2	1.18	.71	.90	.06	—.18
B.	1	1.06	1.10	.78	.46	.35
	2	.86	.54	.31	.31	—.04
C.	1	1.49	1.46	1.35	.84	.47
	2	1.75	1.47	1.27	.92	.56
H.	1	2.16	1.59	1.51	1.00	.73
	2	1.92	1.90	1.81	1.56	1.08
Pi.	1	1.57	1.42	.92	.52	.29
	2	2.00	.82	.69	.98	.18
Pr.	1	3.30	3.07	2.57	1.78	1.10
	2	1.29	.87	.55	.09	—.22
G.	1	2.09	1.56	1.29	1.03	.24
	2	1.88	1.74	1.29	1.06	.77
Pp.	1	2.02	1.81	1.53	.95	.74
	2	1.97	1.64	1.16	.95	.71
Hn.	1	1.98	1.88	1.50	1.30	.89
	2	2.03	1.55	1.43	1.31	.56
Average.		1.74	1.41	1.18	.80	.44
$E \times D^2$		157.03	141.00	130.09	96.80	74.36

The first three mentioned had some knowledge of optical illusions and the first two were acquainted in part with the hypothesis upon which I was working. The others had no knowledge of the nature or object of the experiment except that which was gained as a result of their own observation in the progress of the same.

Turning to an examination of the results shown in the tables we find that, with a very few exceptions, there is a uniform decrease in the influence of the secondary stimuli corresponding to an increase in the distance between the primary and secondary stimuli. The majority of the exceptions to be noted will be found in Table II., in which are shown the results for primary stimulus 17.0 cm.

TABLE II.

INFLUENCE OF TWO SECONDARY STIMULI, EACH 2.0 CM. LONG, UPON A PRIMARY STIMULUS 17.0 CM. LONG AT DISTANCES 10.0, 10.5, 11.0, 11.5 AND 13.5 CM.

Distances.		10.0 cm.	10.5 cm.	11.0 cm.	11.5 cm.	13.5 cm.
Subject.	Series.	Influence of Secondary Stimulus in cm.	Influence of Secondary Stimulus in cm.	Influence of Secondary Stimulus in cm.	Influence of Secondary Stimulus in cm.	Influence of Secondary Stimulus in cm.
E.	1	1.15	.75	.93	.25	.44
	2	1.42	1.11	.53	.15	.00
Pa.	1	.73	.97	.57	.20	.19
	2	.96	.68	.72	.40	—.16
B.	1	1.06	.95	.46	.05	—.18
	2	1.13	.93	.61	.28	.27
C.	1	1.74	1.45	.64	.23	—.60
	2	1.54	1.31	1.42	1.12	.50
H.	1	1.70	1.60	1.18	1.04	.18
	2	1.68	1.49	1.60	1.28	.89
Pi.	1	1.05	.57	.56	.37	.18
	2	1.41	1.01	.44	.07	—.09
Pr.	1	1.75	1.61	1.49	1.00	.66
	2	1.67	1.08	1.11	.41	.46
G.	1	1.94	1.27	.90	.71	.20
	2	1.41	1.22	1.16	.81	.98
Pp.	1	1.83	1.58	1.06	.83	.46
	2	1.62	1.39	1.17	1.02	.40
Hn.	1	1.79	1.41	1.45	.99	.47
	2	1.76	1.26	1.17	.62	.45
Average.		1.47	1.18	.91	.59	.29
$E \times D^2$		147.00	130.09	110.11	78.03	52.85

The only explanation which I can offer for the greater irregularities manifest in Table II. is the fact that a stimulus 17.0 cm. cannot be distinguished with certainty from either 18.0 cm. or 16.0 cm. and inasmuch as the 17.0 cm. stimulus in the order of the experiment follows sometimes the 16.0 cm. and sometimes 18.0 cm. stimulus, the judgment when it related to the 17.0 cm. stimulus was unequally influenced by the preceding

TABLE III.

INFLUENCE OF TWO SECONDARY STIMULI, EACH 2.0 CM. LONG, UPON A PRIMARY STIMULUS 18.0 CM. LONG AT DISTANCES 10.5, 11.0, 11.5, 12.0 AND 14.0 CM.

Distances.		10.5 cm.	11.0 cm.	11.5 cm.	12.0 cm.	14.0 cm.
Subject.	Series.	Influence of Secondary Stimulus in cm.	Influence of Secondary Stimulus in cm.	Influence of Secondary Stimulus in cm.	Influence of Secondary Stimulus in cm.	Influence of Secondary Influence in cm.
E.	1	1.43	.71	.28	.19	—.17
	2	1.53	1.23	.77	.27	—.23
Pa.	1	.64	.45	.29	.41	.16
	2	.76	.62	.41	.29	—.28
B.	1	1.32	1.12	1.25	.60	.58
	2	1.50	1.19	.67	.50	.01
C.	1	1.76	1.58	1.27	.86	.01
	2	1.33	1.17	.91	.90	.45
H.	1	2.18	2.11	1.60	1.16	.18
	2	2.55	2.40	1.80	1.37	.98
Pi.	1	.82	.66	.35	.26	—.15
	2	1.99	1.78	1.19	.78	.02
Pr.	1	3.15	3.02	1.61	1.11	1.34
	2	1.23	.92	.49	.59	—.07
G.	1	2.40	1.96	1.66	1.37	.16
	2	2.06	1.41	.89	.51	.53
Pp.	1	2.30	2.46	1.98	2.01	1.60
	2	1.49	1.13	1.08	.46	.25
Hn.	1	1.38	1.19	1.51	.57	.63
	2	1.42	1.02	.68	.52	.30
Average.		1.66	1.41	1.07	.75	.32
$E \times D^2$		183.01	170.61	141.51	108.00	62.72

judgments relating to the 18.0 cm. and the 16.0 cm. stimuli. The subject was particularly liable to such confusion, because she was not informed as to the number of primary stimuli which were used, nor that a primary stimulus of different length was always introduced when the cards were changed.

Considering the general averages of all results for each distance, we find that the irregularities referred to have been eliminated and a consistent decrease in influence corresponding to each increase in distance is manifest. For example, the influence of two secondary stimuli, each 2.0 cm. in length, acting upon a primary stimulus 18.0 cm. in length at a distance of 10.5 cm. is found to be 1.66 cm.; at distance 11.0 cm. the influence of the same secondary stimuli is 1.41 cm.; at distance 11.5 this influence has diminished to 1.07 cm.; at 12.0 cm. distance the influence is 0.75 cm. and at 14.0 cm. it is 0.32 cm.

influence. This same degree of regularity is manifest in each of the other tables.

An attempt to establish anything like an exact proportion between the decrease in influence and the increase in the square of the distance was a failure. It became at once apparent that the decrease in influence was far more rapid than the increase in the square of the distance.

The foregoing fact directed attention to another principle, viz., the intensity of visual stimuli decreases as the stimulus is moved toward the periphery of the retina. There are not, within my knowledge, any recorded experimental data which directly confirm this last statement, and, indeed, the well known device of the astronomer of using the periphery of the retina in order to bring to view an otherwise indiscernible star seemed at first thought in direct contradiction to such a statement. This astronomical devise, however, only shows really that the periphery of the retina may under favorable conditions, be more sensitive to very *faint* stimuli than the fovea centralis and can be explained by the fact that the fovea centralis being constantly bombarded by intense stimuli becomes insensitive to very weak ones.

On the other hand, the common facts of every day experience that we see most distinctly when the stimulus falls upon the center and less distinctly when it is moved toward the periphery, together with the well known facts of nerve distribution upon the retina afford sufficient confirmation of the statement that the intensity of the same objective stimulus decreases as the stimulus is moved toward the periphery.

Applying this principle to the case under consideration, we see that when the secondary stimuli are removed to a greater distance from the primary stimulus, they are removed at the same time towards the periphery of the retina, inasmuch as the eye maintains the same position relative to the primary stimulus. We have, therefore, at the same time, an increase in distance and a decrease in intensity of the secondary stimulus, although it remains objectively the same length. Both of these factors thus entering into the conditions of our experiments call for a decrease in the influence of the secondary stimulus, according

to our hypotheses, and the very rapid decrease to which attention was called is, so far, in confirmatory of rather than contradictory to this hypothesis.

If our hypothesis is valid, it follows that the influence exerted by a stimulus A multiplied by the square of its distance would equal the influence of a stimulus B multiplied by the square of its distance. In other words, $E \times D^2 = C$, in which E represents the influence of any secondary stimulus, D is the distance of that stimulus and C is a constant.

As has already been shown, the value of C in the results previously recorded is not constant. For example, in Table I. the value for the five distances decreases from 183.01 to 62.72. This rapid decrease was due to the very rapid diminution of the value of E, and this last we have attributed to the decreased intensity of the 2.0 cm. stimulus occasioned by its removal towards the periphery, in addition to the increased distance.

In order to compensate for this decrease in intensity of the secondary stimulus, I prepared a new series of figures in which the same primary stimulus and the same distances were employed as in the former experiments, but the length of the secondary stimulus was altered. The amount by which the secondary stimulus should be altered in length was determined as follows: I selected arbitrarily one value of C, viz., that shown in the second column of each table of results. I then determined for each distance what the value of E should be, using the value of C selected as a constant. I was thus enabled to determine what effect a secondary stimulus of the same subjective intensity should have at different distances.

Now at a given distance, we know by experiment the effect of a secondary stimulus 2.0 cm. in length; we also know for the same distance, by computation as above shown, what the effect of a secondary stimulus of a certain standard intensity ought to be. The problem is to determine how much the secondary stimulus shall be lengthened or shortened in order that it may have the same subjective intensity as the standard.[1]

For lack of a better, I adopted the purely objective method of solving this problem, using increase in objective length as

[1] Compare Weber's Law.

equivalent to increase in subjective intensity. For example, referring to Table I., we select the influence of a secondary stimulus of 2.0 cm. at a distance of 11.0 cm. as the standard. The value of C (see second column) in this case is 170.61. We have assumed that this value should be a constant, if the intensity of the secondary stimulus remained constant. But we find that the value of C when the distance of the 2.0 cm. stimulus is 11.5 cm. (see third column) is only 141.51, the actual influence of secondary stimulus being only 1.07 cm. Now if the value of C were constant the actual influence of secondary stimulus ought to be 1.29 cm., provided the intensity of our 2.0 cm. stimulus had remained the same. This conclusion is reached as follows: The value of C should be 170.61, but as a matter of fact it is only 141.51. This indicates that the influence of secondary stimulus (1.07 cm.) is less than is to be expected of a secondary stimulus equal in intensity to that one which we have selected as the standard (second column) and, indeed, 1.07 cm. is as much less than the influence of a secondary stimulus of standard intensity ought to have been, as 141.51 is less than 170.61. In other words (170.61 × 1.07 cm.) ÷ 141.51 = 1.29 cm. which is what the influence of a seconday stimulus of standard intensity ought to be at the distance 11.5 cm. Further, if a secondary stimulus 2.0 cm. in length has produced an effect of 1.07 cm., how long must the secondary stimulus be in order that it may produce an effect of 1.29 cm.? Proceeding according to the objective method, this question is answered by the following arithmetical operation: (1.29 × 2.0 cm.) ÷ 1.07 cm. = 2.41 cm., which last is the length which our secondary stimulus must have at distance 11.5 cm., in order to be equal in intensity to the standard, which is a 2.0 cm. stimulus at distance of 11.0 cm. Proceeding according to this method, I calculated, upon the basis of results given in the three precding tables, what the length of the secondary stimulus should be in our new series of figures, in order that a standard intensity might be maintained throughout. The method of procedure is unquestionably crude, and is justified only on the ground that it was used merely as an empirical device. It is doubtless possible to determine definitely the relation between increase in subjective inten-

sity and increase in objective length of visual stimuli. When this is done it will doubtless be possible to construct a series of figures, in which the secondary stimulus at different distances remains of the same subjective intensity. The time at my disposal did not admit of such a determination.

The validity of the objection just raised against the method of constructing the new series of figures was fully justified by the results of the experiments made with these figures. These results are shown in Tables IV., V. and VI. In the case of Table VI., primary stimulus 18.0 cm., the addition to length of secondary stimulus has produced a result which gives to *C* a practically constant value. But in Tables IV. and V. the value of *C* appears in a constantly diminishing ratio, showing that

TABLE IV.

INFLUENCE OF SECONDARY STIMULI OF VARIOUS LENGTHS, BUT ESTIMATED TO BE OF THE SAME SUBJECTIVE INTENSITY OR VALUE, UPON A LINE 16.0 CM. IN LENGTH AT THE SAME DISTANCES SHOWN IN TABLE I.

Lengths of the Secondary Stimuli.		1.79 cm.	2.00cm.	2.15 cm.	2.90 cm.	3.70 cm.
Distances.		9.5 cm.	10.0 cm.	10.5 cm.	11.0 cm.	13.0 cm.
Subject.	Series.	Influence of Secondary Stimulus in cm.	Influence of Secondary Stimulus in cm.	Influence of Secondary Stimulus in cm.	Influence of Secondary Stimulus in cm.	Influence of Secondary Stimulus in cm.
E.	1	1.10	.71	.69	.68	—.06
	2	1.36	1.17	1.08	.95	.70
Pa.	1	1.15	.93	.88	.70	.36
	2	.88	.82	.48	.54	.07
B.	1	1.28	.93	.75	.67	.48
	2	1.21	.91	.62	.63	.35
C.	1	1.37	1.31	1.05	.77	.60
	2	1.36	1.14	.72	.69	.48
H.	1	1.27	1.04	.80	.67	—.09
	2	1.30	.96	.73	.43	.21
Pi.	1	1.40	1.34	1.20	1.06	.89
	2	1.60	1.61	1.42	1.22	1.08
Pr.	1	1.92	1.66	1.22	1.11	.50
	2	1.72	1.39	1.25	.99	.80
G.	1	2.00	1.64	1.55	1.43	.51
	2	1.29	.56	.69	.34	.39
Pp.	1	1.63	1.44	1.25	.88	.63
	2	1.40	1.34	1.20	1.06	.89
Hn.	1	1.55	1.36	1.05	1.01	.89
	2	1.79	1.80	1.67	1.34	.96
Average.		1.43	1.20	1.01	.86	.53
$E \times D^2$		129.06	120.00	113.52	104.06	89.57

the addition in length was not sufficiently large. In other words we have a somewhat new verification of Weber's Law, viz., equal increments in objective length of visual linear stimuli do not imply equal increase in subjective intensity of the visual stimulus.

Comparing Tables IV., V. and VI. with I., II. and III. respectively, we find that the increase in the length of the

TABLE V.

INFLUENCE OF SECONDARY STIMULI OF VARIOUS LENGTHS, BUT ESTIMATED TO BE OF THE SAME SUBJECTIVE INTENSITY OR VALUE UPON A LINE 17.0 CM. IN LENGTH, AT THE SAME DISTANCES SHOWN IN TABLE II.

Lengths of the Secondary Stimuli.		1.77 cm.	2.00 cm.	2.35 cm.	3.32 cm.	4.90 cm.
Distances.		1.00 cm.	10.5 cm.	11.0 cm.	11.5 cm.	13.5 cm.
Subject.	Series.	Influence of Secondary Stimulus in cm.	Influence of Secondary Stimulus in cm.	Influence of Secondary Stimulus in cm.	Influence of Secondary Stimulus in cm.	Influence of Secondary Stimulus in cm.
E.	1	1.22	1.12	1.03	.97	.42
	2	1.23	.90	.81	.72	.43
Pa.	1	.80	.84	.41	.23	.02
	2	.89	.65	.40	.25	.15
B.	1	1.20	.85	.86	.58	.48
	2	1.24	1.08	.77	.57	.33
C.	1	1.47	1.28	1.02	.93	.30
	2	.77	.87	.62	.64	.20
H.	1	1.70	1.54	1.50	1.50	1.16
	2	.88	.75	.72	.71	.51
Pi	1	1.14	1.07	1.03	.97	.95
	2	1.00	1.01	.80	.45	.27
Pr.	1	1.61	1.39	1.35	1.04	.96
	2	1.00	.75	.86	.58	.50
G.	1	1.76	1.49	1.29	1.38	.84
	2	1.10	.79	.77	.48	.35
Pp.	1	1.77	1.46	1.28	1.35	.71
	2	1.53	1.02	1.06	.91	.34
Hn.	1	1.98	1.63	.93	.98	.68
	2	1.20	1.19	.94	.69	.58
Average.		1.28	1.08	.92	.80	.51
$E \times D^2$		128.00	119.07	111.32	105.80	92.95

secondary stimulus has greatly increased the constancy of C, in other words the effect of the secondary stimulus of increased length has been uniformly greater. It appears, therefore, more than probable that if the length of the secondary stimulus were increased according to subjective rather than objective standards,

the value of C would become really constant as is demanded by our hypothesis.

TABLE VI.

INFLUENCE OF SECONDARY STIMULI OF VARIOUS LENGTHS, BUT ESTIMATED TO BE OF THE SAME SUBJECTIVE INTENSITY OR VALUE, UPON A LINE 18.0 CM. IN LENGTH, AT THE SAME DISTANCES SHOWN IN TABLE III.

Lengths of the Secondary Stimuli.		1.85 cm.	2.00 cm.	2.41 cm.	3.54 cm.	5.43 cm.
Distances.		10.5 cm.	11.0 cm.	11.5 cm.	12.0 cm.	14.0 cm.
Subject.	Series.	Influence of Secondary Stimulus in cm.	Influence of Secondary Stimulus in cm.	Influence of Secondary Stimulus in cm.	Influence of Secondary Stimulus in cm.	Influence of Secondary Stimulus in cm.
E.	1	1.41	1.35	1.19	1.29	.48
	2	1.39	1.25	1.22	1.30	1.18
Pa.	1	1.52	1.40	1.25	1.15	1.08
	2	1.34	1.23	1.06	.97	.72
B.	1	1.61	1.36	1.26	1.10	.95
	2	1.40	1.24	1.59	1.08	.98
C.	1	2.02	1.92	1.76	1.57	1.35
	2	1.39	1.30	1.14	.83	.84
H.	1	1.32	1.36	1.30	1.18	1.10
	2	.81	.64	.48	.40	.13
Pi.	1	1.28	1.34	1.27	1.18	1.02
	2	1.66	1.47	1.37	1.25	1.28
Pr.	1	1.93	1.64	1.72	1.65	1.47
	2	.99	.80	.58	.57	.64
G.	1	1.48	1.07	1.35	1.06	1.02
	2	1.11	1.18	.84	.68	.43
Pp.	1	1.28	1.40	1.36	1.34	1.07
	2	1.91	1.87	1.59	1.45	1.18
Hn.	1	2.02	1.55	1.21	1.02	1.18
	2	1.37	1.26	.94	1.12	.63
Average.		1.46	1.32	1.22	1.11	.94
$E \times D^2$		160.96	159.72	161.34	159.84	184.24

For purposes of comparison, I present three tables, VII., VIII. and IX., showing the results of some experiments conducted by Misses E. Dickson and B. Brock, students in my laboratory course. In these experiments only four subjects were used and ten tests of each was made with each figure. Thus each of the general averages represents forty judgments.

The primary stimulus in these experiments was 24.0 cm., 25.0 cm. and 26.0 cm., respectively. The secondary stimulus was objectively 2.0 cm. in length, the same as in the experiments previously recorded, but inasmuch as the increased length of the primary stimulus makes it necessary to remove the

secondary stimulus towards the periphery, the subjective intensity of the secondary stimulus is materially decreased as compared with the secondary stimuli of the experiments previously discussed.

Comparing these results with those of Tables I., II. and III., we find the same general features, viz., decrease in inverse proportion to distance, but a decrease more rapid than is demanded by increase in square of the distance.

Detailed comparison of results in the several tables, bring out some interesting relations. For example, in Table I., where we have primary stimulus 18.0 cm., secondary stimulus 2.0 cm. and distance 14.0 cm., the influence of secondary stimulus is 0.32 cm. In Table VII. for the same distance, but a primary

Table VII.

Influence of Two Secondary Stimuli, each 2.0 cm. Long upon a Primary Stimulus 24.0 cm. Long at Distances 13.5, 14.0, 14.5, 15.0 and 16.0 cm.

Distances.	13.5 cm.	14.0 cm.	14.5 cm.	15.0 cm.	16.0 cm.
Subject.	Influence of Secondary Stimuli in cm.	Influence of Secondary Stimuli in cm.	Influence of Secondary Stimuli in cm.	Influence of Secondary Stimuli in cm.	Influence of Secondary Stimuli in cm.
A.	1.4	.80	1.0	.9	.7
Kg.	2.2	1.2	.8	—.2	—.7
Ch.	1.2	1.1	.9	.8	.6
Cl.	1.1	.9	.8	.6	.0
Average.	1.5	1.0	.9	.5	.15

stimulus of 24.0 cm. the influence is 1.0 cm. Similarly, comparing the effect when primary stimulus is 25.0 cm., and the same distance, 14.0 cm., we find, in Table VIII. an effect of

Table VIII.

Influence of Two Secondary Stimuli, each 2.0 cm. Long upon a Primary Stimulus 25.0 cm. Long at Distances 14.0, 14.5, 15.0 and 15.5 cm.

Distances.	14.0 cm.	14.5 cm.	15.0 cm.	15.5 cm.
Subjects.	Inflnence of Secondary Stimuli in cm.	Influence of Secondary Stimuli in cm.	Influence of Secondary Stimuli in cm.	Influence of Secondary Stimuli in cm.
A.	.7	.8	—.2	—.7
Kg.	1.2	.8	.5	—.4
Ch.	1.6	1.3	1.0	.8
Cl.	1.7	1.4	1.0	.5
Average.	1.3	1.1	.7	.05

1.3 cm. Similar relations appear throughout, when the influence in the case of different primary stimuli with secondary stimuli at the same distances is observed. The effect of the secondary stimulus increases not only in proportion to its own intensity, but also in proportion to the intensity, or length, of the primary stimulus.

The Effect of Secondary Stimuli when the Primary Stimulus is Varied.

This relation to which reference was made in the preceding paragraph, was also brought out by a series of experiments especially designed for the purpose. A series of eight cards were prepared as follows: The primary stimuli were 8.0, 9.0, 10.0, 11.0, 12.0, 13.0, 14.0 and 15.0 cm. long; the secondary stimuli were in each case 2.0 cm., in length and the distance between primary and secondary stimuli was in each case 9.0 cm. The only variable factor, therefore in the conditions was the length of the primary stimulus.

Table IX.

Influence of Two Secondary Stimuli, each 2.0 cm. Long upon a Primary Stimulus 26.0 cm. Long at Distances 14.5, 15.0, 15.5 and 17.0 cm.

Distances.	14.5 cm.	15.0 cm.	15.5 cm.	17.0 cm.
Subjects.	Influence of Secondary Stimuli in cm.	Influence of Secondary Stimuli in cm.	Influence of Secondary Stimuli in cm.	Influence of Secondary Stimuli in cm.
A.	1.8	1.8	.6	.7
Kg.	1.7	1.6	1.2	.1
Ch.	1.5	1.3	1.1	.8
Cl.	1.5	1.3	.5	.4
Average.	1.6	1.5	.9	.5

The method of conducting the experiments was in general similar to that already detailed. The cards were presented in the order given above for the first five tests of each series and in the reverse order for the last five tests of each series — ten tests constituting a series. The experiments were conducted by Misses Newton, McConnell and Pauline Smith, three students in the department of psychology. The results are shown in Table X.

Table X.

Influence of Two Secondary Stimuli, 2.0 cm. Long, Upon Primary Stimuli of Varied Lengths, but Same Relative Position and Distance 9.0 cm.

Lengths of Primary Stimuli.		8.0 cm.	9.0 cm.	10.0 cm.	11.0 cm.	12.0 cm.	13.0 cm.	14.0 cm.	15.0 cm.
Subjects.	No. of Experiments.	Influence	Influence	Influence	Influence	Influence	Influence	Influence	Influence
Mc.	100	.24	.38	.40	.37	.51	.52	.61	.94
N.	100	.06	.17	.11	.24	.46	.03	.53	.28
K.	100	.28	.31	.06	.04	.43	.25	.36	.22
Average.		.15	.29	.19	.22	.47	.27	.50	.48

For purposes of comparison, I present also Table XI., which records the results of experiments conducted under exactly similar conditions by Misses Canning and Blalock. In this group, however, the lengths of the several primary stimuli were

Table XI.

Influence of Two Secondary Stimuli, 2.0 cm. Long, Upon Primary Stimuli of Varied Lengths, but Same Relative Position and Distance 14.0 cm.

Lengths of Primary Stimuli.		18.0 cm.	19.0 cm.	20.0 cm.	21.0 cm.	22.0 cm.	23.0 cm.	24.0 cm.	25.0 cm.
Subjects.	No. of Experiments.	Influence	Influence	Influence	Influence	Influence	Influence	Influence	Influence
Bl.	100	.00	.07	.01	.04	.21	.29	.64	.32
Cu.	100	.03	.12	.12	.16	.63	.36	.35	.78
Average.		.015	.10	.07	.10	.42	.33	.50	.55

18.0, 19.0, 20.0, 21.0, 22.0, 23.0, 24.0 and 25.0 cm., and the distance between primary and secondary stimuli was 14.0 cm.

Referring to the summary of results in both tables, it will be observed that in general the influence of secondary stimuli at the same distances increases as the length of the primary stimulus increases. Irregularities, however, in the rate of increase and actual exceptions to the rule are particularly noticeable. The fact that relatively speaking the same inconsistencies appear in both tables would seem to indicate that the order in which the cards were presented was a factor which

affected the result and that the natural ebb and flow of attentive processes was involved. In addition, it should be observed that linear stimuli of the lengths here given cannot be distinguished from one another with any considerable degree of certainty, unless they differ by more than one cm. in length.

We may eliminate these inconsistencies in the two tables by taking an average of three different lengths of primary stimulus. For example: The results in the case of primary stimuli 8.0, 9.0, and 10.0 cm. were 0.15, 0.29, and 0.19 cm., respectively. The mean of 8, 9, and 10 is 9; the average of 15, 29, and 19, is 21. Therefore the corrected result for primary stimulus, 9.0 cm., would show an influence of secondary stimulus equal to 0.21 cm. Similarly the corrected result for primary stimulus 10.0 cm., shows an influence of 0.23 cm. When Tables X. and XI., are corrected according to the method just outlined the average appears as shown Table XII. Here we see a consistent increase in influence of secondary stimulus corresponding to increase in length of primary stimulus.

TABLE XII.

THE RESULTS SHOWN IN TABLES X. AND XI. WHEN REARRANGED AS DESCRIBED IN THE TEXT.

Lengths of Primary Stimuli.		9.0 cm.	10.0 cm.	11.0 cm.	12.0 cm.	13.0 cm.	14.0 cm.	15.0 cm.
Average.	Table X.	.21	.23	.29	.32	.41	.42	

Lengths of Primary Stimuli.		19.0 cm.	20.0 cm.	21.0 cm.	22.0 cm.	23.0 cm.	24.0 cm.
Average.	Table XI.	.06	.09	.19	.28	.42	.46

A closer examination of the two summaries of results shown in Table XII. reveals another fact which has entered as a disturbing element into my experiments and which I have not succeeded in satisfactorily isolating. I refer to the fact that the stretch of open space between the primary and secondary stimuli enters as an element in determining the influence of the secondary stimuli. When variations in the size of this open space are small its influence upon the result may perhaps be disregarded. But in cases of the sort now under discussion, these open spaces play a considerable part. For example, in

Table XII. we see that when length of primary stimulus is increased from 9.0 cm. to 14.0 cm. the influence is increased from 0.21 cm. to 0.42 cm. Now if we accept the hypothesis that this influence varies directly as the product of the intensities of the two sensations,[1] and inversely as the square of the distance between them, then the proportion of increase ought to be readily determined by use of the well known formula for the law of gravity. Using this formula ($f = C\,[(m \times m') \div D^2]$) as a basis of calculation, it will be found that the relation between the influence in the case of the 9.0 cm. and 14.0 cm. primary stimuli, will be as 0.22 is to 0.35. The relation as determined empirically was 0.21 to 0.42. The entire series as determined empirically is 0.21, 0.23, 0.29, 0.32, 0.41, 0.42. The entire series as determined by calculation based upon the formula is 0.22, 0.24, 0.27, 0.30, 0.32, 0.35.

It will be observed that in the latter half of the series there is an increase in the influence of the secondary stimuli, which is in excess of that which is warranted by our hypothesis as represented by the formula in question. It should also be noted that this unexpected increase in influence is coincident with a gradual lessening of the open spaces which separate the primary from the secondary stimuli. In the case of the 9.0 cm. primary stimulus this open space was 3.5 cm., whereas in the case of the 14.0 cm. primary stimulus this space was only 1.0 cm. This would seem to indicate that close proximity of the ends of the primary and secondary stimuli, increases the effect of the secondary stimulus.

The indication just referred to is further emphasized by reference to the other half of Table XII. Here we have primary stimuli increasing in length from 19.0 cm. to 24.0 cm. The series of figures showing the influence of secondary stimulus as determined empirically is 0.06, 0.09, 0.19, 0.28, 0.42, 0.46. A corresponding series calculated upon the basis of the gravity formula would be 0.19, 0.20, 0.21, 0.22, 0.23, 0.24. Here too there is a corresponding decrease in the size of the space which separates the two stimuli.

It is further evident from the foregoing that the disturbing effect of too close proximity of the ends of the two stimuli is in

[1] Intensity of sensation is equivalent to 'sensation mass.'

proportion to the length of the primary stimuli. This relation is manifest in a series of experiments recorded in Table XIII. The experiments were conducted by myself, the subjects being students of psychology. Three subjects were used and results for three series of ten judgments each are shown. In the first column are recorded the judgments of the length of a single line (4.0 cm.) without secondary stimuli; in the second column are shown judgments of the length of the same line when secondary stimuli (2.0 cm.) have been introduced at distance 3.5 cm. The distance between the end of the primary stimulus and the end of the secondary stimulus was only 0.5 cm. The third column shows judgments of a single line 6.0 cm. in length and the fourth column shows judgments of the same line when secondary stimuli have been added at a distance of 4.5 cm. The distance between end points is again only 0.5 cm. And so with each primary stimulus, viz., 8.0 cm., 10.0 cm. 12.0 cm., and 14.0 cm., the distance between primary and secondary stimuli is respectively 5.5 cm., 6.5 cm,, 7.5 cm. and 8.5 cm., but the distance between end points of primary and secondary stimuli is in every case only 0.5. cm.

Referring now to the average of results for all subjects, there is shown a marked increase in influence for each primary stimulus, despite the fact that the secondary stimulus was further removed and probably less intense. It is at once manifest that the formula under consideration cannot be used to determine the relative effect of secondary stimuli in the case of such a series as that which is represented in this table, unless another element can be introduced into the formula. It is not yet clear what this element should be. We can only say that close proximity of the ends of the two stimuli increases the effect of the secondary stimuli and that this increase in effect is itself increased in proportion to length of primary stimulus.

Influence of Secondary Stimuli of Varying Length upon a Primary Stimulus of a Constant Length and at a Constant Distance.

For showing this relation I have not conducted a separate series of experiments, but have rearranged the results shown in Tables I., II. and III. and represent them in Table XIV.

Table XIII.

Influence of Secondary Stimuli 2.0 cm. Long Upon Primary Stimuli of Different Lengths and at Different Distances.

Lengths of Primary Stimuli.		4.0 cm.		6.0 cm.		8.0 cm.		10.0 cm.		12.0 cm.		14.0 cm.	
Distances.		3.5 cm.		4.5 cm.		5.5 cm.		6.5 cm.		7.5 cm.		8.5 cm.	
Subjects.	Series.	Single Stimulus.	Secondary Stimuli.	Single Stimulus.	Secondary Stimuli.	Single Stimulus.	Secondary Stimuli.	Single Stimulus.	Secondary Stimuli.	Single Stimulus.	Secondary Stimuli.	Single Stimulus.	Secondary Stimuli.
E. B.	1	4.04	4.46	5.98	6.52	7.84	8.70	9.85	10.85	12.05	13.16	13.89	15.33
	2	3.89	4.09	5.91	6.12	7.74	8.12	9.74	10.43	11.78	12.53	13.72	14.56
	3	3.86	4.04	5.76	6.35	7.62	8.27	9.57	10.36	11.46	12.47	13.43	14.42
N. B.	1	3.82	3.96	5.84	6.05	7.75	8.24	9.67	10.34	11.68	12.47	13.51	14.25
	2	3.88	4.01	5.92	6.26	7.88	8.36	10.13	10.73	12.24	12.84	14.08	14.82
	3	3.87	4.01	5.84	6.27	7.73	8.28	9.70	10.55	11.68	12.53	13.51	14.65
A. Mc.	1	3.95	4.24	5.85	6.35	7.79	8.44	9.70	10.57	11.67	12.61	13.86	14.50
	2	3.88	4.44	5.87	6.40	7.70	8.69	9.96	10.76	11.93	12.78	13.77	15.30
	3	3.73	3.85	5.61	5.96	7.70	8.30	9.65	10.30	11.75	12.41	13.76	14.69
Average.		3.88	4.12	5.84	6.25	7.75	8.38	9.77	10.54	11.80	12.64	13.72	14.72
Infl. of Sec. Stimuli.			.24		.41		.63		.77		.84		1.00

Unfortunately, the results which are comparable were not obtained in the same series of experiments, but all the results for constant secondary stimulus (2.0 cm.) were obtained first and then results for variable secondary stimulus were obtained in a subsequent series of experiments with the same subjects. Inasmuch as the magnitude of an illusion decreases with practice on the part of the subject[1], we find that in the second series of experiments the influence of the same secondary stimulus under similar conditions is less than in the first series. This is seen by comparing the instances in which the length of the secondary stimulus was the same. For example, when primary stimulus was 18.0 cm., distance 11.0 cm. and length of secondary stimulus 2.0 cm. in both series, the influence in Series 1 was 1.41 cm. and in Series 2 the influence was 1.32 cm., showing a decrease in influence due to practice of 0.09 cm.; similarly, when primary stimulus was 17.0 cm. and secondary stimuli 2.0 cm., influence in Series 1 was 1.18 cm. and in Series 2 it was 1.08 cm., showing a decrease in influence of 0.80 cm.; further, when primary stimulus is 16.0 cm., and secondary stimulus 2.0 cm., influence in first series was 1.41 and in second series 1.20 cm., showing a decrease of 0.21 cm. In comparing the results for the two series therefore we must either subtract these values from the first or add them to the second or, perhaps more accurately, subtract one half from the first and add one half to the second.

Comparisons of individual results of the two series are not satisfactory because of irregularities and we must resort to a comparison of averages in order to discover any consistent relations.

Taking first the results for primary stimulus 18.0 cm. we find the mean of all the distances used is 11.8 cm.; the length of the secondary stimulus used throughout the first series is 2.0 cm.; the mean length of secondary stimulus in the second series is 3.04 cm., the average influence in first series is 1.04 cm. and in second series is 1.21 cm., or if corrected as above suggested the influence in first series is 0.99 cm., and in second series is 1.26 cm. There is thus apparent a more or less direct ratio between the length of the secondary stimulus and its influence.

[1] Cf. Judd, *Genetic Psychology for Teachers*, p. 26.

The results shown in Table XIV. lend themselves, however, to a more comprehensive treatment and enable us to apply directly the formula implied by our hypothesis. By this hypothesis, $f = C(m \times m') \div D^2$ in which f is the force of attraction existing between primary and secondary stimuli, m is the mass or intensity of the primary stimulus, m' is the mass or intensity of the secondary stimulus, D is the distance between primary and secondary stimuli, and C is a constant which must be empirically determined.

From the results of Table XIV. this constant appears to be 0.339, determined as follows: In case of primary stimulus 18.0 cm. $(m \times m') \div D = 0.258$ and the influence as shown above was 0.99 cm. Hence we have $0.99 = C \times 0.258$, or $C = (0.99) \div 0.258 = 0.383$. Determining C for the six possible instances, I found the average to be 0.339 with a mean variation of 0.038.

Using the constant thus determined, it will be found by making proper substitutions that the formula given is an approximate expression for each of the results obtained by experiment, when the conditions are comparable with the foregoing and that the consolidation of individual results increases the perfection of such an approximation.

It should be expressly remarked, however, that the formula with constant above given, cannot be applied indiscriminately to all results in which widely varying distances involving varying intensities of secondary stimuli are included; nor can it be applied successfully to cases in which the ends of primary and secondary stimuli are less far removed from one another than 1.0 cm.

Mean Variations.

It will be noted that the mean variation does not appear in the tables. This is because the results shown in the tables always represent a calculated effect and not a judgment. This effect was determined by subtracting one series of judgments from another. A mean variation parallel to the results shown in the table would have no definite meaning. As regards the judgments made by the subjects I may make the following general statements: For all judgments the mean variation ranged from 0.2 to 0.8 cm. As a rule the mean variation is somewhat larger

Table XIV.

Comparison of Influence of Secondary Stimuli of Different Lengths upon Primary Stimuli of the Same Length and at the Same Distance.

Primary Stimulus 16.0 cm.				Primary Stimulus 17.0 cm.				Primary Stimulus 18.0 cm.			
Series.	Distance in cm. between Primary and Secondary Stimuli.	Length of Secondary Stimuli in cm.	Influence of Secondary Stimuli in cm.	Series.	Distance in cm. between Primary and Secondary Stimuli.	Length of Secondary Stimuli in cm.	Influence of Secondary Stimuli in cm.	Series.	Distance in cm. between Primary and Secondary Stimuli.	Length of Secondary Stimuli in cm.	Influence of Secondary Stimuli in cm.
1	9.5	2.00	1.74	1	10.0	2.00	1.47	1	10.5	2.00	
2	"	1.79	1.43	2	"	1.77	1.28	2	"	1.85	
1	10.0	2.00	1.41	1	10.5	2.00	1.18	1	11.0	2.00	
2	"	2.00	1.20	2	"	2.00	1.08	2	"	2.00	
1	10.5	2.00	1.18	1	11.0	2.00	0.91	1	11.5	2.00	
2	"	2.15	1.01	2	"	2.35	0.92	2	"	2.41	
1	11.0	2.00	0.80	1	11.5	2.00	0.59	1	12.0	2.00	
2	"	2.90	0.86	2	"	3.32	0.80	2	"	3.58	
1	13.0	2.00	0.44	1	13.5	2.00	0.29	1	14.0	2.00	
2	"	3.70	0.53	2	"	4.90	0.51	2	"	5.43	

Primary Stimulus 16.0 cm.

Average Distance 10.8 cm.
Average Length of Secondary Stimulus in Series 1 = 2.00 cm.
Average Length of Secondary Stimulus in Series 2 = 2.31 cm.
In Series 1 $(m \times m') \div D^2$ = 0.253
" " 1 Average Influence of Secondary Stimuli = 1.01 cm.
In Series 2 $(m \times m') \div D^2$ = 0.314
" " 2 Average Influence of Secondary Stimuli = 1.10 cm.

Primary Stimulus 17.0 cm.

Average Distance 11.3 cm.
Average Length of Secondary Stimulus in Series 1 = 2.00 cm.
Average Length of Secondary Stimulus in Series 2 = 2.87 cm.
In Series 1 $(m \times m') \div D^2$ = 0.266
" " 1 Average Influence of Secondary Stimuli = 0.84 cm.
In Series 2 $(m \times m') \div D^2$ = 0.382
" " 2 Average Influence of Secondary Stimuli = 0.97 cm.

Primary Stimulus 18.0 cm.

Average Distance 11.8 cm.
Average Length of Secondary Stimulus in Series 1 = 2.00 cm.
Average Length of Secondary Stimulus in Series 2 = 3.04 cm.
In Series 1 $(m \times m') \div D^2$ = 0.258
" " 1 Average Influence of Secondary Stimuli = 0.99 cm.
In Series 2 $(m \times m') \div D^2$ = 0.393
" " 2 Average Influence of Secondary Stimuli = 1.26 cm.

when secondary stimuli are introduced and the variation is larger when the primary stimulus is increased in length.

Tactual Illusions.

In order to compare the visual and tactual illusions and to show the law of attraction as applied to the latter, I reproduce from the article 'Ueber den Einfluss von Nebenreizen'[1] to which reference has been made, the results of some experiments with a tactual illusion similar to the Müller-Lyer visual illusion. The line was produced by pressure of a thin strip of brass upon the skin of the forearm. The projections or arms were produced by pressure of short brass rods drawn to a point. An apparatus was so constructed that the pressure from line and all points could be given at the same time. For a more detailed description of the method and nature of these experiments, the original article must be consulted.

So far as these results are comparable with results of visual experiments previously detailed, they appear in Tables XV. and

Table XV.

Tactual Illusion. Figure Similar to the Müller-Lyer Figure with Projecting Arms Extending Outward.

Length of Primary Stimulus in cm.	Length of Secondary Stimuli in cm.	Angle Formed by Secondary Stimuli.	Subject Ke.		Subject W.		Subject M.		Average.		$\frac{m \times m'}{D^2}$
			Influence in cm.	M. V.	Influence in cm.	M. V.	Influence in cm.	M. V.	Influence in cm.	M. V.	
6.0	2.9	20°	2.9	0.9	2.5	0.5	3.4	0.4	2.9	0.6	0.90
6.0	5.0	20	3.1	1.0	2.9	0.7	4.3	0.4	3.4	0.7	0.99
7.0	2.9	20			2.5	0.4			2.5	0.4	0.84
7.0	5.0	20			3.4	1.0			3.4	1.0	0.97
8.0	2.9	20	2.2	0.7	2.0	0.2	3.4	0.5	2.5	0.5	0.76
8.0	5.0	20	2.4	0.6	2.5	0.4	4.0	0.3	3.0	0.4	0.94
6.0	2.9	30	2.8	0.5	1.7	0.4	2.7	0.3	2.3	0.4	0.90
6.0	5.0	30	2.3	1.0	1.8	0.2	2.8	0.7	2.3	0.6	0.99
7.0	2.9	30			2.2	0.3			2.2	0.3	0.84
7.0	5.0	30			1.5	0.3			1.5	0.3	0.97
8.0	2.9	30	2.1	0.2	1.2	0.6	3.1	0.9	2.1	0.6	0.76
8.0	5.0	30	2.1	0.7	1.8	0.5	3.4	0.7	2.4	0.6	0.94

XVI. The last column of each of these tables contains the value of $(m \times m') \div D^2$ arithmetically expressed. Here m equals

[1] *Archiv f. d. Gesamte Psychologe*, Vol. I., pp. 31–109.

TABLE XVI.

TACTUAL ILLUSION. FIGURE SIMILAR TO THE MÜLLER-LYER FIGURE WITH PROJECTING ARMS EXTENDING TOWARD THE CENTER.

Length of Primary Stimulus in cm.	Length of Secondary Stimuli in cm.	Angle Formed by Secondary Stimuli.	Subject Ke.		Subject W.		Subject M.		Average.		$\frac{m \times m'}{D^2}$
			Influence in cm.	M. V.	Influence in cm.	M. V.	Influence in cm.	M. V.	Influence in cm.	M. V.	
10.0	2.9	25°	1.9	0.7	1.4	0.2	1.0	0.6	1.4	0.5	0.71
10.0	5.0	25	2.2	0.3	1.5	0.3	1.6	0.3	1.8	0.3	0.89
12.0	2.9	25	1.8	0.4	1.9	0.7	1.3	0.4	1.7	0.5	0.62
12.0	5.0	25	2.5	0.2	1.5	0.0	1.6	0.4	1.9	0.2	0.83
10.0	2.9	45	1.8	0.3	0.9	0.3	0.5	0.3	1.1	0.3	0.71
10.0	5.0	45	1.6	0.3	1.1	0.5	0.6	0.2	1.1	0.3	0.89
12.0	2.9	45	1.6	0.5	1.2	0.2	0.6	0.3	1.1	0.3	0.62
12.0	5.0	45	2.0	0.3	1.3	0.2	0.5	0.2	1.3	0.2	0.83

the length in centimeters of the line or primary stimulus; m' equals the distance of the end point of the projecting arm from the end of the line or primary stimulus. The assumption that this last is the secondary stimulus is somewhat questionable. Its justification, so far as there is any, is based upon the following facts: (1) The introspective evidence of the subjects showed that the end points of the line were most prominent in consciousness, and consciousness of stimulation of the skin between the two end points of the line was very vague and sometimes altogether absent. Consequently the judgment really concerned a distance between two points (corresponding to the line) influenced by a consciousness of a *distance* between these two points and four other points. I have therefore considered these distances the secondary stimulus rather than the actual points stimulated.

D in the formula above given is the distance from center of primary to center of secondary stimulus, measured along the line which consciousness must inevitably follow. In other words D is here one half the primary plus one half the secondary stimulus.

A comparison of the average E (which here represents the average influence of the secondary stimulus or, more properly, the effect of the force of attraction between the primary and secondary stimuli) with the numerical equivalent of $(m \times m') \div D^2$

shows a fairly consistent proportional relation. The accuracy of the proportion is increased when averages of all comparable groups is taken.

It must be observed that comparisons of instances in which the angles are different cannot be made. The intensity of the secondary stimulus is decreased as its angle increases. We cannot compare satisfactorily the results of the two tables for a similar reason.

The figures which are used to represent the intensity of the secondary stimulus are at best only relative, not absolute, and hence the reason that we cannot at present complete the formula and assign a definite numerical value to C.

If, however, comparisons are made of instances in which the conditions upon which the intensity of m' depends are constant, the accuracy of the proportion existing between the attractive forces and the values of $(m \times m') \div D^2$ is very striking. For example, taking data from the first two lines of Table XV., we have the proportion 2.9 : 3.4 :: 0.90 : 0.99 or 3.060 :: 2.871, there being a difference in the proportion of 0.189 cm. But inasmuch as there were four secondary stimuli in the experiments recorded, the actual difference in proportion for a single secondary stimulus would be only 0.047 cm.

The inaccuracy of the proportion in the second half of each table is largely increased. Here the results are from experiments in which the angle of the secondary stimulus was quite large. When this angle was large the magnitude of the illusion was considerably diminished. As a consequence the observer was more liable to be misled by other influences than the immediate objects of perception. Moreover the difficulties of accurately determining the numerical value of the illusion were increased for the experimenter. Hence the values given in the latter half of each table are less trustworthy than the corresponding values in the first half.

Individual Differences.

It is a notable fact that some individuals are more susceptible than others to an illusion of the kind under discussion. Binet has remarked[1] that young children are more susceptible than

[1] *L'Annee Psychologique*, 1894, 'L'illusion d'optique de Mueller-Lyer,' p. 330.

older persons. This fact is also very manifest in the results which are here reported. For example, referring to Tables I., II. and III. a very casual examination is sufficient to reveal the fact that the illusion values for the first four subjects are less than the corresponding values for the remaining six subjects. The first four subjects were adults, while the remaining six were children varying in age from twelve to fourteen years.

This difference may be accouuted for partly on physiological and partly on psychological grounds. In the case of children, the nervous organism is not so firmly 'fixed'; alterations among its parts may be more easily effected. Attraction between the elements of the organism, therefore, has a greater effect.

On the other hand, psychologically speaking, the judgment of the other person is more evenly balanced, which is perhaps equivalent to saying that experience furnishes to the older person a larger supply of data upon which a judgment may be based. A high degree of susceptibility to illusion, therefore, may indicate on the one hand, especially in children, a nervous organism which is plastic and impressible and therefore highly educable, and on the other hand, especially in adults, a weakness of judgment.

In the article 'Ueber den Einfluss von Nebenreizen' to which reference has already been made, I reported experiments by myself upon several groups of children taken from different classes of two elementary schools in Würzburg. The object of these experiments was to determine quantitatively the effect of a secondary stimulus upon the localization of a point stimulated upon the skin of the fore arm. It was found that a fairly consistent parallel existed between the amount of influence exerted by the secondary stimulus and the degree of mental ability attributed to the pupil by his teacher. This parallel was more striking when groups of dull children were compared with groups of bright children. The children used in these experiments varied in age from six to fourteen years. It is very questionable if the same relation would hold for older individuals.

The individual variations in the case of subjects of the same age are marked also in the case of the visual illusion, but the

number of subjects for which results are reported is so small that comparison would be valueless. Future experiment must determine whether or not a relation, such as I have indicated, exists.

Geometric-Optical Illusions.

The literature of this subject is peculiarly rich and not unprofitable. I shall attempt to touch briefly upon such salient features only as are directly related to the phenomena which have been under my observation.

Heymans has shown[1] that the Müller-Lyer illusion (*a*) increases with the length of the projecting arms, is then (*b*) stationary, and finally (*c*) decreases as the length of the arms increases.

These facts which seem to me fatal to most of the theories which have been advanced to explain the illusion, are perfectly in accord with the law of attraction as developed in the foregoing pages. For, increase in length of projecting arm means, (*a*) increase in intensity of the secondary stimulus and (*b*) increase in the distance of the secondary from the primary stimulus. In the former case, we have *increase* in influence and in the latter case *decrease* in influence of the secondary stimulus. If we begin to increase the length of the arms when they are very short, each increment in length corresponds to a relatively large increase in intensity, but as a result of the operation of Weber's law, there comes a time when a very large increase in length of arm (or secondary stimulus) results in only a relatively small increase in intensity. On the other hand, each increase in square of the distance has diminished the influence of the secondary stimulus in proportion.

In the beginning therefore, (*a*) the increase in influence due to increase in intensity is greater than the decrease in influence due to increase in distance, later (*b*) the effect of the two factors is equal, the one counterbalancing the other, and finally (*c*) the decrease in influence due to increase in distance is greater than the increase in influence due to increase in intensity.

Heymans further shows in the same connection (p. 227), that there is a consistent proportional relation between the size

[1] *Zeitschrift für Psychologie und Physiologie*, Vol. IX., p. 236.

of the illusion and the cosine of the angle formed by the projecting arm and the central line (Schenkelwinkel). When the angle increases the illusion becomes less pronounced. This fact harmonizes with our law of attraction, inasmuch as it is to be expected that an attractive force will have greater effect when acting in a straight line than when acting at an angle upon a given object.

The application of the law of attraction to the other geometric-optical illusions with any degree of accuracy is difficult, if at all possible. In general, we may observe, however, that displacements take place in the direction of greater 'sensation masses.' In the Poggendorf figure, for example, the points where the diagonal joins the parallels are drawn, the one upward and the other downward, by the relatively large sensation masses represented by the sides of the two angles formed. The same may be said of the Zoellner figure.

In all such cases, where there is a displacement of a line from its true objective position, it will be found that certain points which mark the direction of said line are acted upon unequally by neighboring 'sensation masses.'

The most difficult factor to determine in all of these phenomena is the value of a particular 'sensation mass,' or, as previously termed, the intensity of a particular visual stimulus. Spatial measurement is a very inadequate expression of this intensity, as we had reason to observe in our discussion of the results of Tables IV., V. and VI. There are evidently several elements which go to determine the intensity of a given visual stimulus. The first of these is undoubtedly spatial size. The second is the proportionate part of all active sensory processes which the sensation in question represents. When an experience is already crowded with sensory elements, the addition of a new element has comparatively little sensory value. A third element is position in the visual field — the same stimulus being more intense upon the fovea centralis than on the periphery. A fourth element is the amount of central reinforcement which may be given the stimulus. Mach says,[1] for example: 'Der

[1] *Pflüger's Archiv*, Vol. 60, p. 509. Also *Zeitschrift für Psychologie und Physiologie*, Vol. 16, p. 298.

blosse Wille rechts zu blicken gibt den Netzhautbildern an bestimmten Netzhautstellen einen grësseren Rechtswerth.' An illustration of both the third and fourth elements just mentioned is furnished by a phenomenon which I have observed in making some experiments with the illusion to which Professor Loeb first called attention. One of the illustrations which Professor Loeb offered was as follows: If one places two pieces of money on a table so that they seem equally far removed to one's right and then places a third piece further towards the right so that the three pieces form a right angle triangle it will be found that the relative position of the first two has been so altered that the lower one which is on a horizontal line with the third, now appears further to the left than the upper one. In my own experience the phenomenon to which Professor Loeb calls attention does not always appear and in fact the reverse phenomenon sometimes appears, *i. e.*, the lower one of the two vertically arranged pieces appears further toward the right. On giving the matter closer attention I found that the change in result was brought about by a difference in the direction of attention. If, for example, the attention is directed to the two lower pieces the third is attracted by both and the phenomenon mentioned by Professor Loeb may be observed. On the other hand if attention is directed toward the one above and the one to the extreme right, or the two forming the hypoteneuse of the triangle, it will be found that the reverse phenomenon takes place, viz., the third is again attracted by the two to which attention is being given, the angle opposite becomes obtuse and the upper of the two pieces which were arranged vertically now appears to lie too far to the left. Here attention, or central reinforcement, and bringing of the two images nearer the fovea, both operate to increase the intensity, or sensation mass, of the two sensations, diminishing in a corresponding degree the intensity of the third sensation. The consequence is that the two stronger attract the third with a greater force than it attracts them and it is therefore displaced from its true relative position.

Finally, a fifth element in determining the intensity of a given sensation is the duration of the stimulus — the intensity of a stimulus diminishing as it grows older. This last element

has perhaps not figured in the case of the illusions which have previously been discussed. But in the case of the so called illusion of reversible perspective it probably plays a large part. Such illusions are usually brought about by staring at a figure. The result of the staring is to diminish gradually the intensity of the sensations occasioned by the points which determine the form of the figure. The intensity of the neighboring points is relatively less affected. Consequently, these neighboring points finally have a larger 'sensation mass' or intensity than the others, and when this happens, they determine the form of the figure according to their own disposition. After this has happened a few times, one can so reinforce the intensity of these latter points from within that he is able to control the phenomenon at will.

THEORIES.

Heymans and Wundt[1] both agree that the Müller-Lyer illusion is due to an almost physical impulse (fast physische Zwang) to follow the direction of the projecting arms with eye movements. One might argue in support of this theory that the law of attraction governs in fixing the strength of the impulse to eye movement. The question still remains unsettled, however, as to whether the judgment is a by-product of this impulse to movement, or whether the impulse to movement is itself a product coördinate with the judgment.

Wundt, in criticism of Heymans' contrast theory, calls attention[2] to the fact that the illusion takes place when either figure is compared with a straight line and no contrast of eye movement is possible. This criticism is justified by my experiments which were concerned with only one type of figure.

It is manifest that such theories[3] as the confluence theory of Mueller-Lyer, the Auerbach indirect vision theory, the Brentano pseudoscopic angle theory, the Thiery perspective theory, the Einthoven dispersion image theory and all others which are based upon phenomena growing out of the extension of arms at an angle, are shown to be inadequate by the fact that the illusion is present when no such angles appear in the figure.

[1] *Physiological Psychology*, Vol. II., p. 149.
[2] *Die geometrisch-optischen Tauschungen*, p. 47.
[3] Titchener, *Experimental Psychology*, pp. 321–328.

General Conclusions.

The law of attraction as represented in the present paper is an attempt to state in definite form a principle which has been more or less prominent in the theories of several writers upon this subject.

Jastrow,[1] for example, has attempted to explain optical illusion in general on the principle that all objects are judged relatively to their environment. Our judgment of a thing is modified by the other things which surround it.

A great variety of facts which illustrate the principle may be drawn from every day experience as well as from experimental laboratories. For example, Lipps[2] calls attention to the fact that cows appear to be larger when they are in narrow, low stalls than they do when outside.

Professor Baldwin in an article upon the 'Effect of Size-contrast upon Judgments of Position in the retinal Field,'[3] reports that a point, in the field of vision, midway between two figures of unequal size, as two squares or two circles, will be attracted towards the larger figure. Further, the tendency to error increases with the relative increase of the side of the larger figure and the tendency is about twice as great when the figures are arranged vertically as when they are arranged horizontally.

In an article entitled 'Normal Motor Suggestibility,'[4] I have reported a series of experiments showing that the localization of a point stimulated upon the skin of the forearm is influenced by the stimulation of a second point, either above or below the one to be localized. It was also shown that the localization of a visual image in the peripheral field was similarly affected by the appearance in the same field of a second visual image, and, similarly, the localization of a sound was affected by a second sound.

The tendency to fuse together of two or more sensations which are simultaneously experienced has been frequently remarked and experimental psychology has shown conclusively

[1] *American Journal of Psychology*, Vol. IV., p. 381.

[2] *Raumaesthetik*, p. 65.

[3] Psychological Review, Vol. II., p. 244. Cf. also the further figures given in his *Fragments in Philosophy and Science*, pp. 275 ff.

[4] Psychological Review, Vol. IX., pp. 329-356.

that one of the chief defects of the older introspective psychology was its failure and inability to recognize in experience the elementary sensations which composed it.

All these and many other similar facts seem to point to a general law of relativity, which may be stated somewhat as follows: ***Every sensation is influenced by every other sensation which may be present in any complex experience.*** The nature of this influence seems to be a direct interaction of one upon the other, the resulting effect of this interaction being determined by the nature of the interacting sensations.

All of the sensations with which I have dealt experimentally are such as to make up 'extensive ideas';[1] they were either sensations defining position or form and magnitude, and the nature of the interaction seems to have been an attractive force, which I was able to measure. This attractive force is governed in its action by the same general law which governs the action of the attractive forces in nature, with which we are already familiar and which has been given mathematical expression in the well-known formula $f = C\,(m \times m') \div D^2$. In its application to the phenomena which have been under our observation, f, in the formula, equals the force exerted by two sensations, the one upon the other, m is the intensity or sensation mass represented by a primary stimulus, m' is the intensity or sensation mass represented by a secondary stimulus, and D is the distance between the primary and secondary stimuli, measured from center to center. The constant C must be determined empirically, and is not the same value in the case of visual and tactual sensations.

The apparently physical nature of the law leads to the suggestion that this attractive force operates between the nervous elements, electro-chemical in nature, which mediate sensation.

On the other hand, one might be justified in admiring that universality of the law, manifest in its consistent operation in two such widely separated spheres as the material and the spiritual.

The time is not opportune, I think for a discussion of the vexed question as to the nature of mind which is involved in the two possible theories here suggested.

[1] Titchener, *An Outline of Psychology*, p. 154.

Summary.

1. When a subject is required to judge the length of a single line, by comparing it with a second line which is variable in length, the single line is always underestimated.

2. Using the same method of comparison, if the subject is required to judge the length of the same line, now accompanied by shorter lines which represent extensions of the line of direction of the original line but which are separated from it by open spaces, the original line is judged to be longer than it was when unaccompanied by the shorter lines, and, generally, it is judged to be longer than objective measurement shows it to be.

3. When the results for several subjects are consolidated it is found that the influence attributable to the addition of the shorter lines, or secondary stimuli, is (*a*) increased when the length of the secondary stimuli is increased, also (*b*) that this influence is increased when the length of the line to be judged is increased, and finally (*c*) an increase in the distance of the short lines from the central lines, or primary stimulus, measured from center to center, is followed by a decrease in the influence of the short lines, or secondary stimuli.

These general relations obtain in the case of individuals as well as for groups of individuals, but the individual variation is somewhat large, and comparisons of individual results are not thoroughly satisfactory.

4. A fourth and a disturbing element in determining the amount of influence of the secondary stimuli was the space between the ends of the primary and the secondary stimuli. When the distance between the ends of the primary and the secondary stimuli was decreased, the amount of influence of the secondary stimuli was correspondingly increased. The exact proportionate relation was not determined.

5. When the disturbing factor just noticed could be disregarded by reason of a favorable arrangement of conditions, it was found that the well known formula, expressing the law of attraction in the material universe, can be applied to the results of the experiments here described.

6. The results of certain experiments in judging the length of lines stimulated by pressure upon the skin of the forearm, also yield to a similar statement.

THE RELATION BETWEEN THE VASO-MOTOR WAVES AND REACTION TIMES.

BY WM. R. WRIGHT,
University of Michigan.

The experiments herein recorded are a series of reaction experiments conducted with the view of ascertaining any possible relation existing between the vaso-motor wave and the reaction time of the subject; or, stated in the form of a question, does the reaction time of the subject vary in length in accordance with the rise and the fall of his vaso-motor, or 'Traube-Hering,' wave?

The subject was placed in a room separate from the recording apparatus so that all distractions of sight and of sound were reduced to the minimum. He was so seated facing a small table that both of his arms rested easily upon the top of the table. Within the palm of the subject's left hand was fastened a Hallion and Comte plethysmograph, while with the right hand he operated a telegraphic key. To the subject was attached also a Sumner pneumograph, the records of which were taken with the view that they might be of special value in the study of another problem in the future. Although no use was made of these records in this series of experiments, it was found that the markings of the pneumograph could be recorded along with the other records without interfering in any way with the subject's attention to the particular task assigned him.

On the table in front of the subject was a telegraphic sounder. This was screened from his view, and furnished the auditory stimulus to which he reacted by pressing the telegraphic key mentioned above.

All the recording apparatus was placed in the experimenter's room, and was connected by air-tight rubber tubing and insulated wires with the apparatus in the subject's room. The records were taken on two kymographs, one of the horizontal

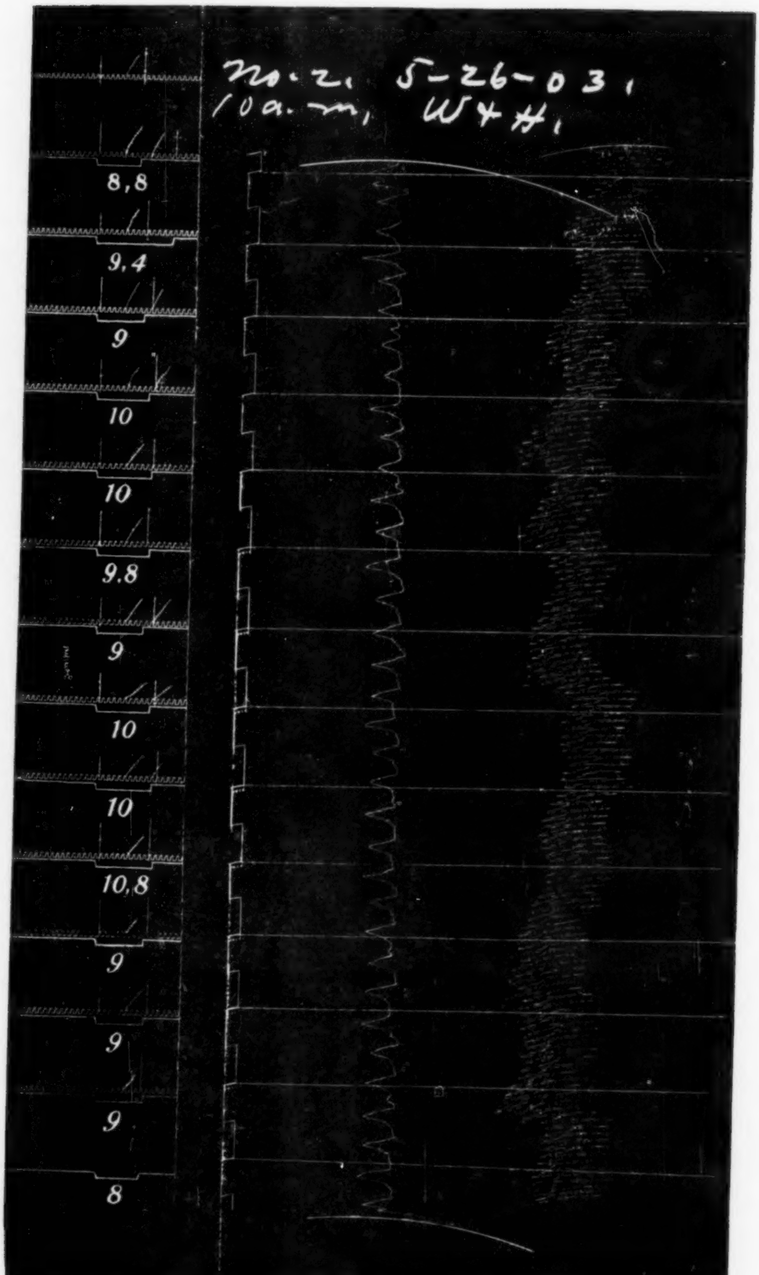
type with a traveling carrier, and the other a Zimmermann, of the vertical pattern. The motive power for the revolving drums was furnished by an electric motor, the horizontal drum, 50 cm. in circumference, being so regulated in regard to speed that it made one revolution in 7.8 seconds. By means of graded pulleys the rate of speed of the vertical drum was so adjusted that the surface of the drum moved at a rate equaling the rate of the longitudinal movement of the markers connected with the horizontal drum; but it was found that complete reliance could not be placed upon this adjustment alone, as the least slip of one of the belts made a perceptible change in the rate of the speed of the drums.

The vertical drum received the markings of a Lombard-Pillsbury piston-recorder which was connected by a rubber tube with the plethysmograph, the markings of a Marey tambour connected by a rubber tube with the pneumograph, and also the records of an electric marker so connected with the reaction time-marker of the horizontal kymograph that the beginning of each reaction was written on the vertical drum. Care was taken to keep the three writing points in the same vertical line upon the drum.

On the carrier of the horizontal kymograph were fastened two electric markers, one, connected electrically with a vibrating tuning fork, marked fiftieths of a second, and the other by its deflections marked the reaction period of the subject. At first a tuning fork of 200 double vibrations was used, but this necessitated such rapid revolutions of the drum that the responses came in close rhythmical succession, and the subject responded when he expected them and not to the signal.

On the pulley of the horizontal drum was fastened a metal attachment that automatically closed and kept closed an electric circuit during one half of a revolution of the drum. The electric marker of the vertical drum, the reaction marker of the horizontal drum, the telegraphic sounder and the telegraphic key were so wired together and connected with the automatic circuit closer that the closing of the circuit gave the subject his signal and recorded on both drums the beginning of each reaction; and the pressing of the telegraphic key by the subject

A B C D



released the reaction marker on the horizontal drum and marked the close of the reaction. For the early experiments the electric current was furnished by storage batteries; but, as these so often proved unsatisfactory, use was finally made of a small dyna-motor which gave a steady reliable current for the tuning fork and the markers.

The only instructions given the subject were that he should keep one position without moving his left hand, and that he should press the telegraphic key with his right hand each time he heard the signal.

Midway between two signals, the release of the telegraphic sounder could be faintly heard, and this became an equivalent for the experimenter's usual 'now.' The length of the time of the revolution of the horizontal drum giving the signals for the reactions was such that the subject was fully able to recover himself before the warning 'now,' and thus there was little fluctuation in the degree of his attention throughout a sitting. Thirteen and occasionally fourteen revolutions of the horizontal drum formed one series of experiments; and, after considerable experience, the experimenter was able to secure three series within an hour.

As the sheets of the kymographs were filled, each was fixed by the usual bath; and, for convenience in reading the records, the sheet containing the vaso-motor waves was pasted across the reaction sheet in such a manner that, for an ocular demonstration, the joining of the points marking the close of the reactions formed a series of curves under the vaso-motor curves (see Plate II.). The readings were taken by measuring in fiftieths of a second the length of each reaction. By means of lines drawn perpendicular to the line connecting the points marking the beginning of the reactions, the exact positions of the reactions in time with reference to the vaso-motor waves were found. The lengths of the reactions, or the reactions in seconds, were then grouped into four groups, as to whether the reaction occurred at the base of the vaso-motor wave, on the rise of the wave, at the crest of the wave, or during the fall of the wave. The reaction times of each group were then averaged according to the number of experiments in each group. A correction of .016

second was made to cover the latent period of the reaction marker.

Five persons served as subjects, Dr. Pillsbury (P.), Mr. Hayden (H.), Mr. Freund (F.), Miss Lee (L.) and Mr. Wright (W.). With the last named, Mr. Hayden conducted the experiments.

Experiments with P. were conducted between nine and ten o'clock, a. m., and the table below shows the results obtained.

	Number of Experiments.	Average Reaction Time in Seconds.
Base of vaso-motor wave,	113	.170
Rise of vaso-motor wave,	65	.186
Crest of vaso-motor wave,	90	.194
Fall of vaso-motor wave,	82	.187

H.'s reactions were taken at ten o'clock, a. m., two o'clock, p. m., and four o'clock, p. m. on different days. His records, given below, are considered first as forming one series irrespective of time; then each hour is represented as making an independed series.

	Number of Experiments.	Average Reaction Time in Seconds.
Base of vaso-motor wave,	53	.249
Rise of vaso-motor wave,	63	.261
Crest of vaso-motor wave,	42	.270
Fall of vaso-motor wave,	56	.262

EXPERIMENTS CONDUCTED AT TEN O'CLOCK, A. M.

	Number of Experiments.	Average Reaction Time in Seconds.
Base of vaso-motor wave,	14	.241
Rise of vaso-motor wave,	24	.262
Crest of vaso-motor wave,	11	.283
Fall of vaso-motor wave,	15	.254

EXPERIMENTS CONDUCTED AT TWO O'CLOCK, P. M.

	Number of Experiments.	Average Reaction Time in Seconds.
Base of vaso-motor wave,	7	.249
Rise of vaso-motor wave,	10	.252
Crest of vaso-motor wave,	6	.248
Fall of vaso-motor wave,	17	.261

EXPERIMENTS CONDUCTED AT FOUR O'CLOCK, P. M.

	Number of Experiments.	Average Reaction Time in Seconds.
Base of vaso-motor wave,	32	.249
Rise of vaso-motor wave,	29	.264
Crest of vaso-motor wave,	25	.269
Fall of vaso-motor wave,	24	.266

F.'s records, taken at 8 o'clock, a. m., are as follows:

	Number of Experiments.	Average Reaction Time in Seconds.
Base of vaso-motor wave,	9	.334
Rise of vaso-motor wave,	7	.342
Crest of vaso-motor wave,	11	.389
Fall of vaso-motor wave,	8	.299

L.'s record, taken at 2 o'clock, p. m., are as follows:

	Number of Experiments.	Average Reaction Time in Seconds.
Base of vaso-motor wave,	13	.273
Rise of vaso-motor wave,	10	.282
Crest of vaso-motor wave,	19	.293
Fall of vaso-motor wave,	17	.284

W.'s hours corresponded with H.'s and are similarly reported.

Entire Series of Experiments.

	Number of Experiments.	Average Reaction Time in Seconds.
Base of vaso-motor wave,	112	.187
Rise of vaso-motor wave,	58	.189
Crest of vaso-motor wave,	110	.201
Fall of vaso-motor wave,	71	.191

Ten O'clock Series of Experiments.

	Number of Experiments.	Average Reaction Time in Seconds.
Base of vaso-motor wave,	40	.189
Rise of vaso-motor wave,	27	.191
Crest of vaso-motor wave,	45	.208
Fall of vaso-motor wave,	23	.201

Two O'clock Series.

	Number of Experiments.	Average Reaction Time in Seconds.
Base of vaso-motor wave,	47	.190
Rise of vaso-motor wave,	15	.199
Crest of vaso-motor wave,	30	.201
Fall of vaso-motor wave,	24	.186

Four O'clock Series.

	Number of Experiments.	Average Reaction Time in Seconds.
Base of vaso-motor wave,	25	.182
Rise of vaso-motor wave,	16	.175
Crest of vaso-motor wave,	35	.190
Fall of vaso-motor wave,	24	.186

It is to be noted that the reactions followed one another in order at a uniform rate throughout a series of experiments regardless of the position of the vaso-motor wave, and that the subject at all times was ignorant of the relation existing between his reaction time and its relative position with reference to his vaso-motor wave, hence the number of experiments occurring in the different groups, *i. e.*, base, rise, crest and fall of wave, differed widely.

A single sheet of records showed little constancy in its results, and a slight variation may be noted in the breaking up of the whole number of both H.'s and W.'s experiments into the hour series; F.'s and L.'s experiments are too few to be of any special significance; yet even in these cases there is a decided tendency toward the results plainly seen in connection with the whole number of experiments of P., H. and W. The records of the three last named persons show clearly a difference in the times of the reactions that occur at the time of constricted vaso-motors, at the time of dilated vaso-motors or at points midway.

To review P.'s reactions, we find his reactions the shortest (.170 sec.) when the blood supply in his hand is at its lowest, and that his reactions are the longest (.194 sec.) when there is a full supply of blood. Between these two points, the reactions are slightly quicker if they occur at the time of the dilating (.186 sec.) than if they occur at the time of the contracting (.187 sec.) of the vaso-motors.

The results of H.'s and W.'s experiments, taken in their entirety as one series for each, agree with the facts already noted in connection with P.'s reaction times; *e. g.*, H.'s are .249, .261, .270 and .262 seconds, and W.'s are .187, .189, .201 and .191 seconds. These same records when broken up into hour series still show, with one or two slight variations, similar relations. The only real discrepancy in H.'s results is to be seen in the time of his reactions occurring at the crest of the wave, but with this it will be noted that the number of experiments (6) is too small to offset general results. With W.'s hour series but two variations occur, and neither one of these materially changes the general trend of the curve of reaction times established by the greater number of experiments.

In reply to our query stated in the beginning of our report, we would repeat that the results of the above experiments show that the subject's reactions form a curve, which, in shape, agrees close with the curve of his vaso-motor, or 'Traube-Hering,' wave.

L. M. Patrizi in 1896 (see *L'Année Psychologique*, Vol. 3, 1897, p. 359)[1] conducted a series of experiments similar to the above with the exception that he had the opportunity of taking the plethysmographic record from the brain of his subject and thus write the curve of cerebral volume. His reactions are recorded in only two groups, minimum and maximum volume, or base and crest of vaso-motor wave. In all he secured 244 reactions, 128 at the crest and 116 at the base of the wave, and his general average of reaction times shows .3325 seconds for the crest and .345 seconds for the base of the wave — just the opposite of those found in our experiments when the record was taken from the finger.

Notwithstanding the difference found by M. Patrizi in his experiments, he concludes that it is too slight to establish any relation whatever between blood supply and reaction time; whereas our results, conducted upon more subjects and in connection with the blood supply of the hand, do show differences great enough to warrant the assumption that the rate of reaction is related to the 'Traube-Hering' wave.

Were Patrizi more sanguine as to the correctness of his results, it would be interesting to discuss the question of the relation of direction between 'Traube-Hering' of brain and finger; but, as it is, no conclusions on this point can be drawn.

[1] Original article, *Archivi di Psichiatria*, 1896. We have seen only the summary.

ON THE HOROPTER.[1]

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Of all the subjects in physiological optics none has been thrown into greater confusion by conflicting views of different investigators and none has been surrounded by greater mystery than that of the horopter. Helmholtz, after devoting about ninety pages of his monumental work on physiological optics to the horopter, pages replete with experiments and with abstruse mathematical formulæ, evolved a theory which no other investigator could verify even of the few who claimed to be able to understand it. With all this erudite labor and with all the enthusiastic interest of the great philosopher he worked out a single horopter of the infinite number which may exist and even that one, being based on false premises, was absolutely faulty for well adjusted eyes and entirely impracticable for any eyes.

It is, therefore, when all the divergent opinions are considered, not altogether without an appearance of justice that so astute a man as Giraud-Teulon should have characterized the horopter as a 'transcendental fancy.'

"When," he says, "all the labor of determining the surface curve (fulfilling the geodesical condition of the horopter) was ended it was discovered that this surface assumed the form of a *torus*. * * * It was not noticed that a table with four legs, a chair placed before us, were seen singly although they certainly had none of the attributes of a torus."[2]

Nevertheless the subject of the horopter or to put it better, of horopters, is one of great practical importance. We may emphasize the expression and say that it is one of preëminent importance.

[1] Read before the New York Branch of the American Psychological Association and the Section of Anthropology and Psychology of the New York Academy of Sciences, at Yale University, New Haven, October 20, 1903.

[2] *The Function of Vision*, translated by Owen.

A horopter may be defined as consisting collectively of all the points in space whose images, with a given adjustment of the eyes, fall upon corresponding points of the two retinas.

Notwithstanding the view I have expressed of the notable rank which should be accorded to this subject, the general definition as just given is almost the only point concerning the phenomena of horopters on which investigators, those who have conceded a horopter, have agreed.

By some it has been described as a line, by others as a surface and by Helmholtz especially as a most complex and quite incomprehensible combination of curves, planes and straight lines.

Without entering upon the merits of Helmholtz's propositions that the horopter is 'a line of double curvature produced by the intersection of surfaces of the second degree (hyperboloid to a nappe, cone or cylinder)' that 'it is a straight line and a curved plane of the second degree,' etc., we may for a moment, without accepting the doctrine, consider the position of the horopter according to this philosopher when the plane of regard is directed to the horizon.

"In a single case only," says Helmholtz, "is the horopter a surface, it is when the point of regard is situated in the horizontal and median planes and at an infinite distance. The plane of the horopter is then parallel to the plane of regard. * * * In the case of normal eyes thus directed toward the horizon the horopter coincides approximately with the ground on which the observer walks."

If we consider this proposition with care it will appear that if it were correct its accuracy would involve much ocular inconvenience. We do not look at the horizon when we walk. One who would hold the head erect and direct the eyes to the horizon would stumble often in his march. But, according to the proposition, if the eyes should be directed to the ground at a few feet in advance of the pedestrian he would bury his horopter beneath the soil and all the objects in his pathway would appear, so far as a horopter is concerned, confused and indistinct.

I have taken so much of your time with an introduction in order that we may at the outset form an idea of the present state

of the doctrine. Recurring to our definition, if a horopter is the collection of the points in space whose images, with a given adjustment of the eyes, fall upon corresponding points of the two retinas, it follows that horopters succeed each other in endless variety and with amazing rapidity. With every glance of the eyes, with the passing of the line of regard from one part of the page of a book to another, in fact, with every change of the head, of the body or of the eyes themselves and with every degree of convergence a new horopter is developed. A horopter will be formed when the two eyes are so adjusted as to enable the image of the point fixed to be located exactly at the maculas of the two retinas.

The innate impulsion to form a practically complete horopter with any given fixation is so imperious that only insurmountable obstacles will serve as a restraint.

Two tenets or conceptions constitute the essential foundation for the doctrine of the horopter. They are, the theory of the position and direction of the meridians of the retinas and the theory of corresponding points.

In respect to both tenets Helmholtz and most modern searchers in this field have adopted views which have resulted in the confusion in which the subject has been involved.

Before we can proceed to the phenomena of the horopter then it is essential to obtain a correct idea of these two fundamental theories.

We speak of vertical and of horizontal meridians of the retina. They are, like the meridians of the globe, imaginary lines yet they have distinct relation to sight impressions. For example, let us suppose a horizontal meridian passing through the macula, the eye being directed straight forward and the head being in the primary position. The eye fixes a given point the image of which is impressed at the macula. Now if another point at one side of this point of fixation is situated on a higher plane than the point of fixation, its image will be impressed at one side of the macula and below the horizontal meridian. It is unnecessary to consider in detail this doctrine but we may at once assume an understanding of the general principle. Helmholtz, Volkmann, Hering and other investi-

gators came to the more or less uniform conclusion that the horizontal meridians were all parallel with the external horizon but that the vertical meridians were only apparently vertical, and that they leaned out above and approached each other below. Helmholtz's experiments led him to the belief that the vertical meridians of each eye leaned out about $1\frac{1}{4}°$. A number of investigators immediately found that their vertical meridians in each and every instance leaned out exactly $1\frac{1}{4}°$. It remains for a society of psychologists to determine how it happened.

My own researches led me to devise the clinoscope which has now become one of the most important and essential of instruments in practical examinations of the eyes. One of the first things which the clinoscope did was to demonstrate that these leanings are natural defects — personal peculiarities — and that they vary with different individuals from one to a dozen degrees; that it is rare to find two persons in succession who record the same leaning. These leanings I have called *declinations*.

Abundant experience in the correction of these defects of declination have demonstrated beyond all reasonable doubt that the proper position for a vertical meridian is the vertical position.

That Helmholtz had what I have called a plus declination for each eye I am convinced. There is much reason however to believe that it was considerably in excess of $1\frac{1}{4}°$.

Thus, Helmholtz included in his most elaborate mathematical calculations his individual defects which he assumed were physiological features common to mankind.

This was one of the fundamental errors.

The second foundation tenet is the doctrine of corresponding points of the retinas. We may quote Helmholtz's proposition as the accepted view of the doctrine. "*The points which, in the retinal horizons are at equal distances from the point of fixation are corresponding points.*" He states the proposition similarly for the vertical meridians. In fixation with the two eyes the image of the precise point of fixation is impressed upon each retina exactly at the macula or fovea, and, according to the above proposition another point outside the point of fixation

will be impressed upon corresponding retinal meridians which in the case of each eye will be equally removed from the macula. In respect to corresponding points in *the field of vision* we may quote again. "*Corresponding points in the two visual fields are those which are at equal distances and equal in direction from the corresponding horizontal and apparently vertical meridians.*"

While this proposition is not altogether clear it is evident from the context that according to it a series of points equally distant in the field of view and from which proceed lines of direction toward equally distant points in the retinas are corresponding points.

These propositions can not both be true except under circumstances entirely at variance with Helmholtz's illustrative experiments. These experiments are made, not with curved surfaces, hollow spheres, but with plane surfaces like the usual stereoscopic cards or the flat page of a book.

Accepting the experimental illustrations as the only practical tests, the two propositions are inconsistent.

Let us first suppose the distances between corresponding points on the horizontal meridian of the retina are exactly equal. Place a sheet of paper exactly in front of the eyes on which are several points in a straight horizontal line corresponding to the plane of regard (Fig. 1). Let the eyes be fixed on the central point. Then, according to the first proposition these points are not corresponding, for straight lines drawn from them through the two nodal points to the retinas will not form equal angles and will not meet equally removed points of the two retinas.

Suppose our points to be one half an inch distant from each other and fifteen inches in front of the eyes while the two eyes fix the central point, A. Then will the line of incidence passing from the point at the right of the central position, B, form with the line of incidence passing from the point of fixation through the nodal point to the macula of the left eye an angle of $1^\circ\ 54'\ 5''$ while the angle formed by the line of incidence from this same secondary point will constitute with the line passing from the point of fixation to the right macula $1^\circ\ 53'\ 26''$

Passing to the next succeeding point of the series, the incidental line from the new point will form with the line of inci-

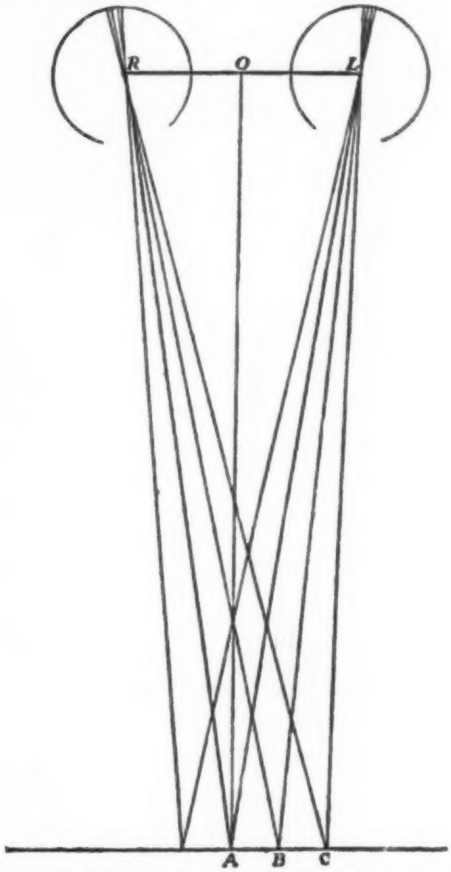

FIG. 1. Let R and L be the nodal points of the two eyes and A the point of fixation. The points B, C, etc., are outside the point of fixation. Suppose

$$RO = 1.25 \text{ in.}, \qquad OA = 15 \text{ in.}$$

$$< RAO = 4^\circ\ 45'\ 49''$$

$$\begin{cases} < ARB = 1^\circ\ 53'\ 26'' \\ < ALB = 1^\circ\ 54'\ 5'' \end{cases} \qquad \begin{cases} < ARC = 3^\circ\ 46'\ 1'' \\ < ALC = 4^\circ\ 39'\ 58''. \end{cases}$$

The points corresponding to the incidence of the lines CR and CL are not thus equally removed from the maculæ.

dence from the fixation point to the left macula an angle of 3° $46'\ 1''$. The incident line to the right eye will form with the

original line an angle of $4° 39' 58''$. It will thus appear that in this example, passing from the macula toward the periphery of the temporal side of the retina the angle increases while on the side of the retina medial to the macula it decreases.

Since these incidental lines cross at the nodal points it is evident that they must extend to unequal distances in the vault of the retina.

Müller, recognizing this, believed the horopter to consist of a circle passing through the nodal points of the eyes and the point of fixation and of a vertical line. This in fact amounts to no horopter. Time does not permit an examination of all these theories and it is sufficient to say that no theory based on equal distances for corresponding points of the retinas can serve as a satisfactory basis for a doctrine of the horopter.

These two tenets on which the whole structure has been erected being rejected the doctrine is to be abandoned or new basic theories must be found.

As already remarked, the clinoscope and practical work based upon its revelations have demonstrated that beyond all question the vertical and horizontal meridians of the retinas are, in typical cases, precisely what their names imply, exactly horizontal and exactly vertical. We may then substitute this fact for Helmholtz's theory and it will serve as our first basic principle.

In respect to corresponding points it is unnecessary to say that there is no such anatomical symmetry as to demand that equal extents on the retinal surfaces should represent equal extents in the field of vision. It is not the fact. It is however, true that there is an innate sense of the goniometrical value of motor impulses directed to the muscles of the eyes, and that the distances between retinal corresponding points need not be symmetrical for the conception of this muscular sense but that nevertheless they bear certain mathematical relations to each other.

We may define corresponding points of the retinas then as, *those points in the retinas which answer to proportional degrees of rotations of the eyes about their centers of rotation, and which, from given points in the plane of the point of fixation receive incident rays which must pass through the nodal points.*

They represent therefore the relation between the muscular and the retinal senses.

The definition is perhaps less easy to the average comprehension than it is to divide the retinas into squares of millimeters and point off so many to the temporal side of one and so many to the medial side of the other retina and call these corresponding points. There are various combined physical and psychical functions which are not to be measured by a pocket rule.

The actual movements of the eyes about the rotation centers are not always essential to an estimation of the relative positions of objects in space. In the absence of the objective movement there is the subjective conception of the impulse required to induce a given movement. We are all familiar with the experiment of Dove in which the observer, looking into a dark box until the eyes are supposed to have assumed parallel directions sees an electric spark within the box and it is seen singly. Of course the impression is made at the temporal side of each macula and there should be by rigid rule, when impressions are at these non-corresponding points, an impression of two sparks seen heteronymously. The rule in this case is not tenable.

If there were actually two sparks there would be two impressions on each retina whereas as a matter of fact there is but one. The consciousness of a single image for each retina and of its position external to the macula leads to the conclusion that a convergence of the eyes would be required to locate the image at the macula and the extent of the required motion would indicate the angle of convergence and therefore the distance of the spark. Of course there are other elements in this complex psychical phenomenon but that mentioned is enough to suggest the course of the psychical process. It is such processes of unconscious conclusions that bring many points within the field of vision into a *subjective* horopter.

Before proceeding directly to examine the principles of the horopter it is necessary to recall some of the changes in the directions of the meridians as the eyes pass from one point of fixation to another. When the point of fixation is at infinite distance and in the median plane all horizontal meridians are horizontal and all vertical meridians are vertical. So also if in the plane of the horizon the point of fixation is brought nearer, the meridians maintain their original relations and these rela-

tions will also continue if the two eyes are directed upward or downward provided the visual lines remain parallel. But if the point of fixation is at such distance as to demand convergence of the lines of regard and if it is above or below the horizon (the head being supposed to be in the primary position) all horizontal and all vertical lines assume new directions. The eyes rotate on their antero-posterior axes. This form of rotation is known as torsion. These torsional rotations are governed by fixed laws and the general principle is known as the law of Listing.

Should the visual lines of the two eyes converge at the same time that the plane of regard is depressed the horizontal meridians of each eye will tilt downward toward the temporal side and upward toward the medial side. The vertical meridians will also tilt with the upper part outward and the lower part inward. The tilting is in every case in proportion to the depression and the lateral direction of the line of vision.

Accepting the two basic principles as they have been stated and with an understanding of the laws of torsion we are in position to examine the phenomena of the horopter, eliminating the mathematical intricacies of Helmholtz and substituting only simple calculations in plane trigonometry. Time will not permit us to inquire in detail into its form in many positions, three will suffice to illustrate the principles and the details of only one of these need be given.

First the observer directs the gaze toward the horizon in the median plane at infinite distance, the head being in the primary position. A horopter is formed at the distance of the point of fixation and it will be a plane surface at right angles to the plane of regard. Objects within or beyond the distance of the point of fixation will not be in the objective horopter but may be in what we may term a subjective horopter. They may be impressed on the two retinas and they will appear, as in the case of the spark in the Dove experiment, as single, the principles controlling the psychical phenomena in that experiment being here modified to meet these different conditions.

Second, if the gaze is directed somewhat downward and to a point a few feet in advance, as in the case of a person walking,

the horopter will still be very nearly at right angles to the plane of regard, tipping forward slightly since, although there is depression (a negative ascensional angle) of the plane of regard, the convergence (the lateral angle) is so slight as to induce small torsional action and the principle of objective and subjective horopter may be applied as in the first case.

There is also, at the lower part of the field of view a bending in of the horopter so that more of the pathway is in the horopter than would be were it through its whole extent a plane.

Coming to the third case we may proceed in more mathematical detail.

Let us suppose the case in which the eyes are directed to the page of a book in the ordinary position for reading.

Assume that the gaze is directed so that the point of fixation is in the median plane, and that the plane of regard is depressed 35°. Assume also that the distance between the nodal points of the two eyes is 2½ inches and that the convergence of the eyes (the lateral angle) is for each eye 5°. We have from these data to determine the distance of the horopter and its form and position relative to the place of regard.

To determine the distance of the point of fixation (which will be in the center of the horopteric field) we have the base, 2½ inches and the lateral angles 5°. Taking one half the base

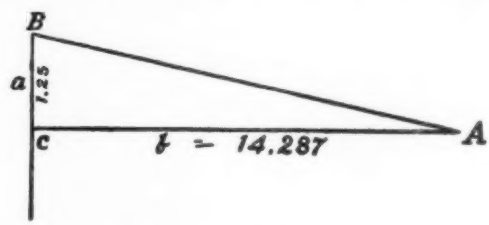

FIG. 2. Angle $A = 5°$, $\frac{b}{a} = \text{Cot } A$, $b = 14.287$.

and one lateral angle we have a base of 1¼ inches, a right angle and an angle of 5° to find the perpendicular or distance from the base line to the page of the book which is readily found to be 14.28 inches (Fig. 2).

The distance being ascertained by the formula $\frac{a}{b} = \cot A$,

a being the base, 1.25 inches, A the angle opposite the base and b the distance sought. At this distance from the base line the image of the point of fixation will be exactly at the macula of each eye.

According to the law of torsions by this depression of the gaze and the convergence the meridians will have left the horizontal and vertical positions. Referring to the table of torsions found in Helmholtz's work[1] we find that for the ascensional angle of 35° and lateral angle of 5° the tilting of the horizontal (and of the vertical) meridians is 1° 35′. These conditions being given what will be the relation of a straight line passing horizontally through the point of fixation across the page to the horizontal meridians of the retinas now tilted 1° 35′ from the actual horizon. A series of points in a straight line thus passing through the point of fixation must impress themselves along the horizontal meridian of each eye otherwise the points will appear confused or double. But how can this series of points in a horizontal line be impressed upon the meridians which are tilted up toward the nasal side each 1° 35′.

It is a most interesting fact that the images of these points will in fact be thus impressed exactly along these tilted meridians of the retina and it is precisely because these meridians of the retina are thus tilted that it is possible for the impressions to be made along the proper meridians.

Too much space would be occupied were we to enter upon a mathematical demonstration of this statement but a little consideration by one familiar with the relation of lines and angles will show that in principle the statement is correct. A demonstration however would show that beyond a certain degree (10° to 20°) in the plane of regard a straight line actually appears to curve.

We come next to the more complicated question in respect to the position of a line running from the top to the bottom of the page. Will this line be at right angles to the plane of regard as the horizontal one is parallel with it or will it lean more or less toward or from the plane of regard?

We may select points above and below the point of fixation and determine their distance from the base line and thus obtain the angle of the surface of the book to the plane of regard.

[1] *Optique Physiologique*, p. 607.

Take, first, a point 5° above and one 5° below the point of fixation. The distance of the point of fixation from the base line connecting the nodal points has already been determined at 14.28 inches. In that case there was a lateral angle of 5° for each eye. Now, since the vertical meridian of the retina tilts out as it rises above the macula this lateral angle will increase as the image is impressed above the macula and it will decrease in proportion to the extent that the impression is made below the macula. Before we can proceed, therefore, it is necessary to find the exact amount of increase and decrease for the selected points 5° above and 5° below the point of fixation, since our angle of convergence will increase in proportion to the extent to which the vertical meridian leans out from its original position exactly at the selected distance and decrease in proportion as the meridian leans in below the macula at the selected distance.

FIG. 3.
Angle $A = 1° - 35°$,
$\frac{a}{b} =$ Tan A,
Tan $A = .02764$,
$b = 5$,
$a = 5 \times .02764$
$= 0° \; 8' \; 52\frac{1}{2}''$.

We may find the extent of removal by the formula:

$a = 5 \times .02764 = 0°, 1382 = 8' 17\frac{1}{2}''$. (Fig. 3.)

$\frac{a}{b} = \tan A$; $\qquad \tan A = .02764$,

In which b is the selected distance above or below the macula, a the required increase (or decrease) in the lateral angle and A the angle of $1° \; 35'$.

This gives .1382 of a degree which is to be added to our lateral angle (angle of convergence) when we can proceed as in the first case to find the distance from the base line to the selected point below the point of fixation (Fig. 4), $\frac{b}{a} = \cot A$, in which b is the distance sought, a the base line, 1.25 inches, A the angle opposite a, $5°.138$. From this we find that $b = 13.904$ inches.

To obtain the distance of the point above the point of fixation we must subtract the $0°.138$ ($0° \; 8' \; 17\frac{1}{2}''$) from 5° when

by the same formula we find the distance to be 14.6976 inches (Fig. 5.)

We have now the distances

5° above the point of fixation	14.6976
At the point of fixation	14.287
5° below the point of fixation	13.905

Forming from these distances two triangles by joining the three lines at their extremities we have a line joining them and

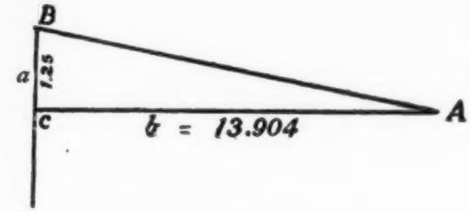

FIG. 4.
Angle $A = 5° 8' 52\frac{1}{2}''$.

forming bases which represent a vertical line in the horopter at the level of the page of the book (Fig. 6).

The acute angle at this surface of the book for the upper

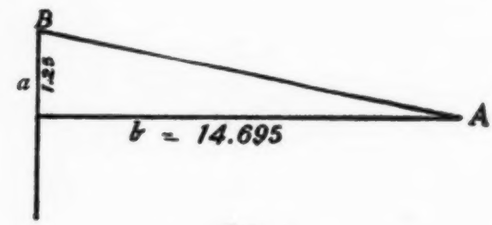

FIG. 5.
Angle $A = 4° 51' 7\frac{1}{2}''$.

triangle of these two is, 69° 38′. That of the lower triangle is 70° 48′ 50.″

We have thus, in the space of 10° up and down the page, a curve of about 1° In other words the horopter in this direction is approximately a plane surface. If the calculation is carried to 10° each way, as in Fig. 6, equal to a space to about five inches on the page of the book the result is nearly the same

but the curve is somewhat greater as we approach the periphery of the field of vision.

This gives us the position of the page in relation to the plane

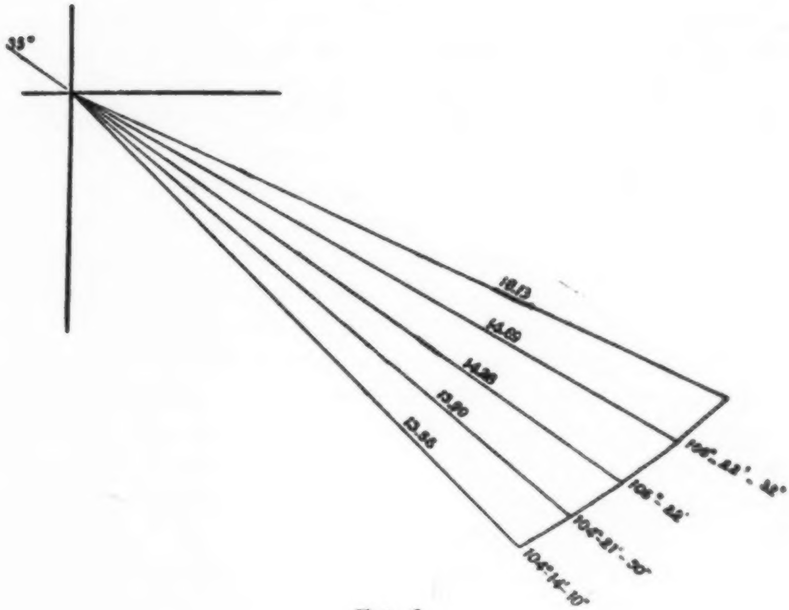

FIG. 6.

of regard in which the horopter is most completely formed and we find that the page is tilted about 15° beyond the right angle with the plane of regard, or at about 105°. We have found only

FIG. 7.

the direction of the horizontal and vertical meridians of the horopter but any other meridian may be found in a similar manner.

An interesting and very simple experiment for those who are able to unite stereoscopic figures by convergence without the aid of a stereoscope beautifully confirms the above calculation.

Draw two vertical lines parallel and at a distance of two and one half inches from each other on a card board (Fig. 7).

Hold the card board so that in fixing the center of the lines the gaze is directed downward 35°. Hold the card board twenty-eight inches from the eyes.

One who is expert with such exercises will be able to unite the two lines at the distance of fourteen inches from the eyes.

If, instead of permitting a perfect union of the lines in the stereoscopic image they are held at about one eighth of an inch asunder it will be easy to find at what angle the board must be held to render the two stereoscopic images exactly parallel.

In my own case I find by numerous experiments and careful measurements that the board must be tilted forward as nearly as can be ascertained exactly 15°.

I have 1° of declination of the right eye which would have little influence on the experiment.

Thus mathematical and experimental research lead to practically the same result in locating this horopter. By the formula given we may locate any horopter in the median plane. In other planes the formula will be modified.

Without discussing the application of these principles to space perception, a field of much interest and in which many empirically known facts in art and in architecture may be analytically tested, only brief time remains to allude to the more practical application of the horopter.

All the discussion which has preceded has been based upon the assumption that the adjustments of the eyes are typical in the sense of being the most favorable to the function of combining the images of the two eyes in a horopter.

In real life anomalous conditions of adjustments, conditions which interpose difficulties in forming perfect horopters, are of extreme frequency.

These anomalous conditions may act as slight hindrances or they may prevent any but an imperfect horopter from being formed.

Let us consider some of these.

It has been seen that with a given depression of the plane of regard and a given convergence a horopter is formed in a position which can be predicated when these two elements and the length of the base line between the nodal points are known. The depression of the plane of regard is controlled by an impulse which is not accidental or ephemeral, but which is automatic and uniform for different persons for the same depression under like circumstances.

Suppose a person whose eyes are so adjusted that with the minimum of impulse to the governing muscles they are directed 8° or 10° of arc above the plane of best adjustment. Among people of New England ancestry this is almost a characteristic as it is with some other groups of people. It is not a disease, it is the normal development from a certain form of cranium.

Suppose again that this person takes a book in hand to read. He holds it in the position and at the distance which we have assumed for our third horopter. Is it not plain that this person must not only depress the plane of regard the 35° assumed, but that he must also induce an additional depression of 8° or 10° as the case may be?

This extra depression at once automatically induces a greater tilting of the meridians. No horopter can then be formed. To remedy the difficulty in a measure the person may throw the head forward 10°, but in so doing there is some disturbance of the equilibrium of the muscles, hence even with this concession the horopter, which will be better than before, may still be somewhat imperfect.

In several papers I have shown that as a matter of fact people with this adjustment of the eyes do throw the head forward and the bending of the neck is, other things being equal, in proportion to the excess of the normal upward direction of the eyes. We will return to this presently.

A second condition which may interfere with the formation of a horopter in the appropriate position is in direct contrast to this. The eyes may be adjusted so that the plane of vision is normally directed low.

Suppose one whose eyes are 10° too low. By the same

reasoning as before we see that because the dynamic depression of the gaze would be less than in the typical adjustment the tilting of the retinal meridians would be insufficient for the horopter and such a person must force the chin high in the air in order to be obliged to depress the gaze sufficiently to induce the necessary torsion.

I have written of these conditions and writers have interpreted the difficulty as a strain on the muscles of depression or elevation.

This is an entire misapprehension. It does not follow that there is any considerable strain on the muscles of adjustment but the head must be placed in position in which the automatic torsions shall in some measure correspond to the direction of the gaze.

A third form of hindrance to the constitution of the horopter is found in the condition which I have called declination.

This consists of an anomalous leaning of the meridians of one or both eyes. It is a very common defect and results in great perplexity to the adjusting muscles. Its practical importance is greater than those conditions already mentioned. It may induce, like the two conditions named, a throwing forward or a tipping backward of the head, depending on the direction, symmetry or degree of the declination defects in the two eyes. All that has been said about the forward and backward holding of the head in the other conditions may apply to these cases and in some instances the unnatural pose of the head and body from this cause are extreme.

What I have to add might perhaps better be addressed to a company of physicians than to psychologists yet in order to comprehend the importance of a subject we must know something of its practical application.

Recall the case of the person whose eyes are adjusted for too high a plane. The head is thrown forward as part of the automatic process of adjustment. The larynx is partly closed, the chest is sunken. Air passes less freely to the lungs than it would were the head held erect. It is among this class of people that consumption commits its ravages. There are few, if any consumptives who do not have a high adjustment of the

eyes or a form of declination which induces a corresponding head position.

Then there is the person whose eyes are adjusted for too low a plane and whose head is thrown back.

It is with this class of persons and with those whose declinations induce a similar pose that the occipital neuralgias, pains in back of the head and neck and in the lumbar region are found. The number of such persons is enormous and the suffering from this cause infinite.

From declinations which do not induce false carriage of the head arise perplexities in adjusting for the horopter which result in headaches, dyspepsias and a long array of nervous ills.

A subject whose importance cannot well be overestimated has been presented in this brief outline in the hope that notwithstanding the necessarily incomplete nature of the presentation, some interest may be awakened among men whose special training peculiarly fits them for more elaborate investigations in this most difficult yet notably practical field of inquiry.

THE LOGICAL AND PSYCHOLOGICAL DISTINCTION BETWEEN THE TRUE AND THE REAL.

It was Mrs. Carlyle (was it not?) who said that 'mixing things is the Great Bad.' To the writer it seems that there is a peculiarly injurious variety of the 'Great Bad' in much of our recent psychological logic. It is because that sort of philosophy which the writer for over fifteen years has been calling 'dynamic' and which now seems to have come to its own under the name 'functional' — it is because, we say, that this kind of dynamic philosophy and functional psychology is peculiarly adapted to correct this 'mixing of things' that the writer offers a few words upon the distinction between the 'real' and the 'true.'

This sounds like a question of definition and a matter for logic to dispose of, but we submit that it is also a question of psychology, and that psychology has already made a distinction (also a matter of definition, to be sure, the facts having been understood from time immemorial) peculiarly adapted to explain the logical distinction here required.

It is remarkable that recent writers seem not to have been aware of the ambiguity arising from the identification of the real with the true. The present writer has elsewhere defined reality as 'affirmation of attribute' and this dynamic statement may usefully be contrasted to Lotze's descriptive definition that 'reality consists in standing in relation.' Upon critical analysis the two statements come to the same thing, but our present method in both metaphysic and psychology requires the dynamic form. Nothing can be real apart from a realizer. As Hoeffding says, 'The real is what we apprehend as real — which, in spite of all effort to the contrary, we must ultimately leave as it is — which we cannot but recognize,' though he at once goes on to confuse this real with what is true.

It may be assumed that all will agree with our definition of simple reality as a statement of metaphysical reality. Dewey says: 'The copula gives the statement of being, asserts the reality.' But he, too, goes on to discuss truth as relational. In our own extended discussion we endeavor to point out the union of subjective and objective in this

identification of essence and attribute, which is only possible in an active percipient.

The logical abstraction of 'pure being' as the activity of the subject apart from the content (meaning — *i. e.*, attribute) is possible, but it involves, as Hegel abundantly showed, the loss of reality. Pure being and non-being were in this sense the same, both being all one to the subject who demands the *act* of asserting or identifying as well as the *mode* asserted.

Professor Baldwin has made, as we intimated above, the important distinction between psychic and psychological, and both Professor Bawden and the writer have shown that the psychic cannot become the subject of scientific analysis. Nevertheless it does afford the foundation on which science (the psychological) must rest. The predicate of reality pertains and can pertain only to the psychic. We do not construct reality but simply perceive (affirm) it. This ultimate fact in experience is reality. The opposite to real is not false, it is non-existent or unreal.

If it be objected that this limitation does violence to common usage it must be replied that any necessary logical distinction may do the same. The distinction between psychic and psychological traverses ordinary usage from end to end but if it expresses a true distinction it is well worth while to reconstruct terminology. In fact, it may well be that any further great advance in psychology must wait for a wholesale reformation of terminology.

The point is that we must have a word for this primary feeling-cognition which we have called reality. Reality is not something we say about experience but a *quale* of experience itself. We ascribe truth to *relations* of things or events among themselves, or ultimately as parts of a universe of things and events. Any reality would be no less real if it existed alone. If we must use Lotze's definition of reality as a 'standing in relation' we should say reality grows out of a relation to the subject alone, but this is a metaphysical after-thought.

When the naked fact of experience comes to be thought about or, in Baldwin's language, becomes psychological, we begin to develop relations which are true or false in so far as they do or do not cohere in an organized whole. The whole duty of science is so to cause the facts thought of to cohere in an organization. This is the sphere of truth.

There is a sense, however, in which reality escapes from the limitation of the psychic and sits enthroned over all thinking. In last analysis elements of our thinking have to be verified by reference to

real experience. Sometimes we get a long way from such experience in abstract thinking. We keep building one set of relations upon another, trying with all our powers, meanwhile, to keep these relations true among themselves, much as one might work out the orbit of a comet, but at last the test is whether things in experience stand back of the true relations — whether the comet can be really found in this orbit.

When Höffding says: "The evidence of reality is given, then * * * in the firm connection of percepts. We can never be so strongly convinced of the reality of single things and occurrences, as of connected series of things and occurrences," he has confused reality and truth. Compared with the earlier statement quoted above, the incongruity appears grotesquely. He said that, 'in spite of all effort' we 'can't help' recognizing reality, and that there can be no question of any other than this subjective criterion, and now he proposes to add to this once-for-all reality greater reality by multiplying relations. But this is just the difference between truth and reality. Reality, once realized, can by no possibility be improved upon or made more real, while, on the contrary, truth grows more certain the more nearly all known relations are found to cohere with the given relation.

It is not meant by this, of course, that the truth increases with the number of instances, as in the common logical fallacy, but truth becomes more convincing the greater the scope of interaction discovered. The truth that all Felidæ are carnivorous is not greatly increased by observing one cat repeatedly to eat meat nor by seeing that one kind of cat always eats meat, but the finding that a different species of animal combines feline dentition with a carnivorous habit adds greatly to the evidence by proving that certain combinations are non-essential and throwing into prominence the organic or genetic relations.

Bosanquet seems to state the law of reality in the definition: 'Logic treats of the mental construction of reality,' 'the world which surround him is there only as an idea, *i. e.*, only in relation to something else, the consciousness which is himself.' But immediately and, indeed as in duty bound (his subject being logic) he proceeds to discuss the true. For him the objective world is 'what we are constrained to think in order to make our consciousness consistent with itself.' In other words, reality consists in consistence of relations, which is precisely truth. Logic might be defined as the science of truth.

Perhaps the discrimination of reality from truth may even help in the much discussed problem of the subjective and objective. For ex-

ample, when we discover (by a round-about means) that a presentation has been made to consciousness we also get directly (subjectively) an affirmation of attribute. This is an ultimate of experience. It does not make an external world. 'Light is,' and that is all there is about it. But when I, psychologically, accumulate a lot of data and construct the concept of *substance*, this is a matter of relation. The brightness, heat, weight, etc. are made to cohere in the substance, 'candle,' a thing projected out of self and, by implication at least, contrasted to self, as an object. All these relations of activities are true to the extent that they cohere in one system or organism. When the question arises in our metaphysics as to the truth of the objective world as a whole, as it will when we become aware of the subjectivity of all knowledge, there is but one answer — the one already used. The objective world is true because it is in one organism with the subjective mind. Just as our partial judgments are true or false as they prove to be founded on relations in one whole, organically, not to say causally, connected, so the larger judgment 'there is a true external world apart from the mere act of perceiving it' is true only if the percipient or perceiving force be organically part of the same universe. No other criterion is possible.

The feeling of reality comes from the immediateness of the elements of experience.[1] It defies analysis and requires no definition and yet is implicit in all practical life. The judgment of truth, on the other hand, is a fluctuating evaluation based on relations which are known rather than felt. The weight of evidence forces me to believe what is true, I require no evidence to cause an experience to be real nor will any amount of evidence lessen its reality.

The old illustration of the inability of the blind to realize visual data though they may weave about them all sorts of relations, of the truth of which they are fully convinced, may not be realizable by the non-blind. To this end let us take another example.

A friend of mine who is expert in both physics and physiology, informs me seriously and in detail that he has discovered that by using the radium waves β, and passing them through a set of refracting appliances, he is able to produce a series of irritants which, when applied to the sensitive nerve plexus in the hollow of the human foot, give rise to sensations unlike any other. They possess a great keenness and penetrating force and seem to vibrate throughout the organism by a process of excessive irradiation. Each of these sensations has the

[1] Cf. Baldwin's explicit treatment of 'Reality-feeling' in distinction from 'Belief' in his *Feeling and Will*, Chap. VII.

peculiarity of localization in certain parts of the body. One 'wave-length' causes irritation at the root of the tongue and marked increase of blood supply. Each is also accompanied by its own emotional response, so that one kind of stimulus predisposes to religious fervor and exalted egoism and the other causes morose and turbulent passions. One even produces a violent desire for something of which no concept can be formed. Now I may believe all these as true statements of fact but they do not nor can they produce in me any sense of reality such as five minutes of actual experience might produce.

The writer believes that a consistent limitation of these words to the spheres respectively indicated will lighten the burden of the student of metaphysics as well as of psychology.

The loose use of the words real and true in psychology coupled with clear consciousness of the distinctions involved is encountered in James' Psychology. "The sense that anything we think of is unreal can only come when the thing is contradicted by some other thing of which we think. *Any object which remains uncontradicted is ipse facto believed and posited as absolute reality.*" But the only thing that can never be so contradicted is immediate experience. A subsequent experience may *explain*, it can never *annul* it. The only things that can so be contradicted are judgments of relations. The presentation 'rain-bow' is real, but the judgment 'rain-bow now in the sky' can be proven untrue.

If the word 'real' be considered to have too strongly intrenched itself in the wide sphere in which it has been used so carelessly, surely a new word is required for the primary affectation of consciousness called 'sense of reality' and 'reality-feeling.' The further characterization 'a sort of feeling more allied to the emotions than anything else' may, perhaps better apply to the recognition of truth. The reason for this relation to the feelings will be found in the nature of feelings. The writer in his inhibition-irradiation theory of pleasure-pain (which has recently received a psychological restatement by Fite[1] as 'resistance theory') has attempted to derive all emotional acts, physiologically considered, from resistance, obstructions, depletion, or other interference with the flow of nervous impulses, so that there is irradiation or inhibition respectively. If this derivation be correct it will follow that all acts of identification must share in this peculiarity. The new concept meets a barrier at the threshold of recognition which is finally thrown down and the wave of thought finds outlet in a path of least resistance, it is identified with previous acts. This release affords the

[1] PSYCHOLOGICAL REVIEW, X., 6.

condition for pleasure. Identification in one form or another, is back of nearly all intellectual pleasures. Discovery of a true relation is accompanied by pleasure, failure to identify is painful.

It is not without interest in this connection to observe how easily and satisfactorily the dynamic (functional) psychology disposes of the confusion expressed in the classical discussion between nominalism, realism, and conceptualism. So long as precepts, recepts, Anschaungen, concepts, and the like, are conceived as possessions or contents of the mind this discussion is inevitable, but when we become fully aware that these are names for acts or parts of processes the difficulty disappears.

When a mode is perceived there is a simple psychic act, even though the stimulus be of the most varied character. Here we have to draw a line as important as any in psychology. We, from the outside as observers, say that a stimulus has been perceived, but what we actually did was to affirm a mode (quality, attribute). Subsequent (psychological) activities consist in combinations of this material into relations. The act of perceiving does not posit any relation (unless the implicit relation to the subject be so considered, and this is thought back into the psychic and is a matter of metaphysic and not of psychology). Psychological work is all apperceptive; its processes are all synthetic (even its analyses). What Romanes calls a 'recept' is a thinking together of percepts. This unifying work of consciousness is a function of its unity which, as an equilibrium, is organically necessary. All organization must unify.

Here, for example, is a roll of paper passing through a ruling machine armed with many pens. I load one pen with blue ink and from that time forward a blue trace moves along the paper along with the red, green, and black traces. I may shift the adjustment here and there and these traces are brought into various relations, forming patterns, etc. The initial inking is the perceiving. This process adds to the activities in the mind a new one which may be shifted, combined, and modified in various ways but never thereafter will the mind-process-group be the same as it would have otherwise been. The psychic equilibrium has been changed. The relations between the several percepts is infinite but some of these are employed instead of others in our constructive thought. Out of activities, all of which cohere in an organism, our selection of part and our conception or thinking together is more or less an act of violence and must always so remain. In so far as a teleological nexus is formed the thinking together is true, in so far as the union is a purely arbitrary one or non-teleological, it is false.

For example, in our classification I have a concept, 'gopher-genus *Geomys*.' Another naturalist has another idea of the limits of the genus. Our concepts may be equally true but this truth consists in both cases in the recognition of a teleological bond. He perhaps includes more of the segment of evolution or career of the 'gopher movement' in nature than I do. The difference is nominal, the agreement is conceptual. We may not say in either case that *Geomys* is a *real* thing but it does in both these cases represent a *true* concept. Let the generic limits of *Geomys* once be set and agreed to, I then place in the genus an animal proving to belong to another line of descent, the reference is false. It is a question of relation.

It is wrong to say that a concept is only a name. It does exist in nature as the subjective expression of the truest thing we know and the most important. It is a 'genetic' verity. It is a career — a doing, in relation with all doing. It is a teleological verity.

But, it may be said, we are only holding a mirror up to nature and see the trajectory of a flying bird, for example, momentarily depicted thereon, or we are but exposing a sensitive plate in a telescope and get only a bright trace thereon. But these illustrations do not go far enough. In order that the plate may receive the star trace correctly, the mechanism of the telescope must follow the path of the star. There must be coördination. So our concept is a conceiving or following of the trajectory of nature. The proof of correctness is exactly the interaction. Our conceptualism has, therefore, a link to realism in that only upon the assumption that we are part of the organism from which the stimulus comes could these correspondences become intelligible. When we no longer find the trace on our photographic plate we adjust the movement. The feeling of reality and the conviction of truth have their justification in the monistic construction of organism.

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THE PERIOD OF CONVERSION.

The recent scientific study of religious experience has led to many interesting and important results, not the least of which is the relation between conversion and the period of most rapid growth. But the difficulties attending the study of such phenomena are evident here as elsewhere. Among these difficulties may be mentioned that of dealing with a very complex group of mental phenomena without any adequate method of controlling their conditions. The usual method

employed is the 'questionnaire,' supplemented more or less by hypnotic and other experiments. The chief reliance must be placed upon answers given at a distance, to questions which may or may not have been perfectly understood, by persons more or less accustomed to scientific observations and dependent upon their memories of events which transpired in the past. Several obvious sources of error lie in this method. First, the unreliability of memory and second, the unreliability of the observer's judgment as to the meaning of the question and as to its proper answer. But a third objection and this time a purely psychological one, arises from the complexity of the phenomena under investigation. Professor Wundt used to say in private conversation (but whether he has ever put it in print the writer is not able to say), that one can seldom be sure as to the meaning of answers secured by the questionnaire method, because of the complexity and consequent variability of the factors entering into the observer's judgment. Descriptive data of a more or less exact character are the most that can be hoped for.

But this matter of investigating religious experience is beset by two further difficulties which threaten its scientific value. To be thoroughly scientific, experiments and observations must be of such a nature as to be repeated by others; while in these matters, both the significance of most of the questions asked and the interpretation of the answers are alike beyond the control of other investigators. Consequently, the valid results of work in this field are fewer in number than might be expected considering the number of investigators and the extent of their efforts.

These difficulties are illustrated in the investigations as to the period of most frequent conversions. Different writers do not seem to take the term conversion in the same sense. To one author it means the change in human character by which *any* set of religious ideas and aims become the center of a man's life; to another it is that change in man's character by which Christian ideas and aims become central in his life. Now, for the psychologist, Christianity is a set of aims and motor attitudes belonging to religion as its genus; Christianity is a species of religion. Men may become religious without being Christians although they can not become Christians without becoming religious.

With this distinction in mind let us look briefly at this question of the period of conversion. By religion in general let us understand a natural consciousness of relations to a Totality of Existence of which we are a part and upon which we depend, together with the beliefs,

types of action and institutions which have grown out of this consciousness. Lower animals and young children are not religious because they have no notion of themselves as subjects of experience, They do not think of themselves as personally identical, as capable of right and wrong conduct and of laying plans for the future. A man may think of himself as an animal and regulate his conduct accordingly; he may think of himself as a member of a certain church, family, club or political party; and in each case his thought of himself and his belief about himself are the most important factors of control in his conduct and character. If it were possible for me to determine your thought of yourself I could afford to let your conduct take care of itself; if I had absolute control over your idea of yourself, I could let your religion take care of itself. This reflective thought of self is a man's recognition of his peculiar place and function in the totality of life out of which he has arisen, to which he is organically related and upon which he depends. He may conceive himself, as Tolstoi says, as a spirit passing through a series of existences so related to each other that his conduct in one existence determines his position in the next. He may conceive himself to belong to a people chosen by God to receive protection and blessing, upon condition of perfect obedience to God's commands. He may think of himself as one of the thoughts and purposes of an Absolute Being who is perfectly rational and perfectly good. However he may conceive the Total Existence of which he is, in some sense, a part, and however he may define his position in it, this conception and definition together with the emotions and conduct growing out of them, constitute his religion, his faith. Conversion, in the naturalistic sense of the term, is that inner change by which some such conception and definition, constituting a set of religious ideas and aims, become central in a man's personality.

Now I suppose it is conversion in this sense which has been found to be one of the regular phenomena of adolescence, usually occurring somewhere between the ages of twelve and twenty-five, the year of greatest frequency being the seventeenth. Accordingly, the period of most frequent conversions seems to come just after the periods of greatest brain-weight and of greatest increase in body-weight. Professor Starbuck's conclusion is that the periods of conversion and the periods of most rapid bodily growth tend to coincide.

I suppose the most important result of these investigations is the conclusion that conversion, in some broad sense, is the normal experience of every man, marking the transition from his childhood to his youth, or that from youth to his manhood. It is the step by which an

adequate sense of selfhood is approached and made possible. Conversion as a natural phenomenon is a deepening and broadening of one's ideas of himself which is at the same time a deepening and broadening of one's ideas of others; it marks the entrance into a new life based upon a profounder view of the kind of being one is, of the class of beings one belongs to. Conversion is an affair of the social consciousness; by it a youth comes to feel that he belongs to a noble company and to a divine order of things; by it he enters at last upon real life in a real world.

It has been objected that this period for the greatest frequency of conversions comes too early and that conversions frequently occur far on in life. Certain it is that a large part of the work of the church has been directed in vain to the converting of adult men, if the period mentioned above is the only period of possible conversions. Now with reference to this objection, aside from the doubt which attaches to the conclusions of Starbuck and Coe as a result of the methodological difficulties mentioned above and the relatively small number of cases examined, I should say that the word conversion is here used in our two different senses. As used in these investigations it seems to me to mean the natural coming to himself which every normal man, be he Hindoo or Hebrew, Latin or Greek, at some time or other experiences. To those who object to the results of these investigations it means the accepting of Christ and Christian truth as the central religious facts of the universe. Just what view of life and what attitude toward the world a youth is converted to will depend upon the training and surrounding influences of his life; that is, it will depend upon imitation and suggestion. He may by conversion become a Hindoo, a Mohammedan, or a Christian; he may be converted, like Tolstoi, to a simple faith which he understands to be the universal essence of all great religions; he may be converted to some country and make patriotism his religion; he may be converted even to some calling so that henceforth he defines his relation to the world as that of a miner or a teacher of English Literature, or a preacher of the gospel of Christ. I believe there are men in whose lives patriotism or devotion to some calling have, for a time at least, nearly all the essentials of a religion. That an individual should undergo conversion is insured by the laws of mental growth and the conditions of social intercourse; but that he shall become a Christian by conversion must be insured by teaching, preaching and living Christianity. Hence the significance of the church, of family worship and of all those noble agencies through which Christian truth is taught and Christian attitudes are trained.

That Starbuck and Coe must be nearly right as to the time of conversion in this general sense of the term, I think there can be but little doubt. For consider what the adolescent period is. It seems necessary to keep saying that puberty is not the whole of it, nor even its most important part. At about the tenth year girls begin to grow more rapidly than they have ever grown since infancy. Something like a year later, boys start, outstripping the girls about the fifteenth year and ultimately attaining the larger stature of the two. They first shoot up like iron-weeds, then broaden out and then fill up. At about fourteen in girls and fifteen in boys the brain weighs more than at any other time in life. During this period new organs develop, new instincts and acquired reactions show themselves, new centers in the sympathetic and central nervous systems develop and begin functioning. Moreover, growth is never proportional throughout the body. One organ after another and one nerve-center after another starts into activity and then subsides; and with these spasmodic developments, the youth's interests flash up and die away. Now he will be a great poet and artist, now a great orator and statesman, now a great adventurer and desperado or a great naturalist or a great inventor. And all the time he is living under an enormous blood-pressure and the demands of his growing organs are draining the energies of his central nervous system. If a youth does not discover himself at this time of disappointment and growth and trial, at what time in life is he more likely to do so? As a matter of fact the mental characteristics of the period are just what we should expect — a deepening awareness of self such that all experiences, especially in girls, come to have an intensely, sometimes a morbidly personal reference; an insatiable craving for sympathy and comprehension from those who are older; an extravagant passion for self-sacrifice, and a certain fickleness and fancifulness of interests, ambitions and tendencies. That reflective self-awareness develops especially at this time, is shown by the fact that when this craving for sympathy and this impulsive self-sacrifice are not satisfied by wholesome family and social relations, certain morbid types of self-consciousness are apt to result. Such morbid types are manifest in the desires to enter monasteries and nunneries, to become trained nurses and heroes of tragedy, and to commit suicide rather than endure the awfulness of living. Here also belong the feelings of many youths and maidens that they are different from other people, that they are hopelessly bad and alien, that no one understands them, and that God (if there be a God) has somehow left them out of His great plan for His world. The more one studies

the mental states of adolescent boys and girls and the more one comprehends their fierce doubts, their titanic yearnings and their tremendous burdens of anxiety and fear, the more one is convinced that this is the natural time for the great awakening. At this time, and particularly just after the period of greatest bodily growth when the energies of the central nervous system are no longer drained to supply the demands of developing organs, one should come to realize one's place in the experience of God and in the institutions of His world.

But the term conversion as used by some recent writers means that change in man's religion by which Christ comes to be its center. If the foregoing view is true, every normal human being should undergo conversion some time within or near the adolescent period; every mature man is in this sense of the word religious; but to become religious in this sense is obviously not the same thing as to become a Christian. Two classes present themselves, viz., those who in their early conversion become Christians and those who in their early conversion become religious without becoming Christians. In the latter class I do not see why conversion to Christianity may not occur at any time after the adolescent period. In this second sense of the term, conversion may occur many times in the course of a life; and it is the almost universal experience of ministers and teachers that conversion does occur at any age from the twelfth year until death.

It is in the belief that the term conversion is used in these two different senses indiscriminately by different writers of the recent scientific movement in the study of religious phenomena, that the two ought to be kept distinct especially in dealing with the question as to the periods of conversion, and that their confusion has resulted from the complexity of the problem, the limited number of cases examined, and the necessary inadequacies of the method used, that this article is written. To the writer it seems idle to condemn *in toto* all efforts to attain exact knowledge as to the psychology of religious experiences, and we see no reason why recent efforts in that direction should not be welcomed and encouraged. But it seems to be a field in which results must finally be reached by deduction from general psychological hypotheses which are yet to be inductively established in other departments of the science. Meanwhile such deductive procedure is aided by such facts as Leuba, Starbuck, Coe and James are seeking accurate, systematic and exhaustive accounts of.

As to the time of conversion, two separate lines of investigation suggest themselves as extensions of the work already so faithfully done in a field where, both from the point of view of the science of psy-

chology, from that of the practical worker in education, and from that of the conscientious individual seeking an intelligent control of his judgments in religious matters, light is so sorely needed. First, an elaborate series of investigations carried out in different lands among persons of different religious beliefs for the purpose of comparing the religious experiences of people in different countries, climates, races and civilizations. Secondly, a series of investigations carried out by teachers and ministers of different persuasions in Christian countries for the purpose of determining the times, the conditions and the nature of conversions to Christianity, and to other types of religious conviction. The difficulties of such investigations, from a scientific point of view, are enormous and results can only be contributory to a future consummation devoutly to be wished. The American investigator would be obliged to work through missionaries and college teachers in remote lands, and this circumstance, in addition to the inherent difficulties of our methods, necessitate the utmost judicial care in sifting results.

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THE GENETIC PROGRESSION OF PSYCHIC OBJECTS.

The recent relatively novel attempts in the literature to approach the logical processes from the genetic point of view, have made it clear that a good deal of close psychology is still needed in this field. What has impressed the present writer is the lack of an actual tracing out of the series of determinations of objects at the successive stages of cognitive development, and the motives in each such progression from one 'psychic object' to the next. This is the topic to which I am here applying the title of this short paper. The term 'progression' is one which I have used in a somewhat technical sense elsewhere[1]; it denotes a real genetic movement from one mode or stage of development to another.

In a series of university lectures, which are to be revised for chapters in a forthcoming work on the genetic treatment of the logical operations, I have worked out a tentative scheme of the sort; and as its points of emphasis are not altogether those hitherto familiar, I venture to present it here for preliminary criticism; hoping as well that it may incite to a renewed discussion of the general topic.

[1] Psychological Review, May, 1903. I use the term 'object' in the sense of whatever consciousness means or intends — that is whatever can be in any way, shape, or manner psychically set up, presented, or aimed at. Cf. the writer's *Dict. of Philosophy*, sub verbo.

In a preliminary demarcation of the field, we may ask two broad questions: first, what are the conditions determining the construction of objects at any given stage of mental development; and second, what are the psychic characters of the objects thus determined at any stage. Of course, the treatment of 'any stage' means the treatment of 'every stage,' and that involves the determination of the entire continuous movement of the cognitive function, with the ranging of all the objective determinations or specifications of psychic objects in a genetic series.

In the process of bandying this question about — making it every sort of psychic object! — the following types of enquiry have come to more or less definite shape. If we take the traditional outstanding distinctions of sorts of objects, such as sense objects, objects of memory, of thought, etc., as starting point, we may work out the more evident characters of such objects, range them in their apparent genetic order, and call them, as so arranged, the series of 'objective modes.'[1] We may then endeavor to work out the factors of determination for these modes in succession from the simpler to the more complex, in so doing recognizing any finer distinctions which appear, and rearranging the genetic order as we may find ourselves led to do so. This compels us — or has done so in my own case — to trace out certain relatively independent strands of genetic change, the transformations which certain great phases of psychic process undergo, along with the changes in the objects proper. These accompanying series, in so far as they are essential aspects of what we may call the 'object psychosis,' are indeed necessary to a full statement of the objective progressions. I find it at least interesting therefore — not to make dogmatic statements as to its possible value in each case for the main problem itself — to distinguish in the actual results to which I have been led, the following phases of consciousness,[2] traced in each case along with the objects, through a series of modes in turn: (1) the controlling conditions of the determination (that is, the 'control' of the object, a problem recently made much of in the writings of Professor Dewey), (2) the motive to the

[1] The term mode may be applied, I think, both to the sort of function whose progressions we are tracing out, and also to each characteristic stage in these progressions themselves, *e. g.*, the thought mode is a stage in the development of the cognitive mode.

[2] This description of these series was drawn up in answer to a question raised by one of my students after the table (Table I., herewith) in which the results were spread out, was presented on the blackboard; I say this to avoid the suggestion that the lines of enquiry were worked out under any prearranged scheme. On the contrary, the different modal series, as they may be called, resulted directly from the attempt to analyze and trace out the objective determinations in order.

determinations each in turn (the problem of 'interest,' which I find of extreme importance in the later discussions as to the determination of 'truth,' as 'practical,' or 'theoretical,' or both), (3) the function involved in each determination (the sort of attention in which the actual interest finds its vehicle), (4) the meaning of the object, over and above its actual objective marks (here the question of 'logical meaning' is of course uppermost, and with it what I call the problem of 'individuation,' or range of application — in logic, 'quantity' — and the question also of 'real reference,' or the psychic meaning of 'reality').[1]

With so much statement of the problem and the way of approaching it, the accompanying Table I. is I think fairly intelligible. The remarks upon it which follow are meant to explain the more unfamiliar features of the results which it exhibits, including the definition of certain new terms which it has been found necessary to use.[2]

As to the successive objective determinations themselves (1 to 8 in col. i.), they are largely explained by what is found in other columns of the same level, in each case. The principal innovations in the series consist in the essential progression from Memory objects (2) to Judged or Logical objects (7) through the stages represented by Fancy (3, in which arises the dualism of 'inner-outer'), Play (4, characterized by the constructions of 'semblance' or 'make-believe' — col. iii. — and 'experimental control' — col. vii. —) with the important transition, through the rise of psychic control and quasi-logical classification (col. iii.) to the dualism of 'mind-body,' called the 'Substantive mode' (5). In fact, I am prepared to insist that what is here called the 'Semblant' mode is an absolutely necessary term in the rise of the great dualisms which make the logical consciousness possible.[3]

Another point to be remarked is that the dualism of 'Self—not-self' (6) is made germinal to Judgment (7), and that the former carries with it as also preliminary, and so available to say the least, the

[1] Of course many other questions might be asked about the objective consciousness, as *e. g.*, what its emotional coloring, its conative accompaniments, etc., but these might just as well be asked in the reverse form — in tracing out the progressions of feeling or conation. Here we are dealing with what is found to be necessary in (1) the *determination* and (2) the *characterization* of the object *qua* object.

[2] As to the new terms, they are of minor importance, of course, and need not claim to be 'fittest'; the suggestions of others would be most welcome on this as on other features of the matter.

[3] Of course this and the other essential features of the progressions, so far as in any degree novel, are argued in detail in the full treatment, which is to appear in a volume from the press of Messrs. Swan, Sonnenschein & Co., London.

TAB

Psychic Progressions.

MODES.

i. *Objective.*	ii. *Logical.*	iii. *Individuation.*	iv. *Reality.*
1 Objects of Sense. 2 Objects of Memory.	1 [Pre-logical.] 2	1 Cognition of object. 2 Recognition.	1 Reality-feeling of 1. The Present. 2. The Persisting. 2
Image objects 3 Fancy Objects. (inner-outer)	3	3 Recog. of Unity Difference. Psychic Group.	3 Unreality-feeling Existence (inner-outer).
4 Play Objects.	4	4	4 Semblance of 1. Inner Imitation. 2. Einfühlung. (Make-believe).
5 Substantive Objects. (mind-body)	Quasi-logical. 5	5 Recog. of Class, Group, Concrete Plurality.	5 'Prac. Judgment.'
6 Content Objects. (self-notself)	6 6	5 Recog. of Gen. Meaning or Intent. 6 Personal. 6 Social.	6 Belief.
7 Judged Scient. (Thought) Theoret.	7 Logical.	7 Recog. of General (concept). Log. Individual. Recog. of Theoret. Universal.	7 Doubt.
8 Moral Objects.	8 Extra-logical.	8 Recog. of Gen. Self. Moral Individual. 8 Recog. of Pract. Universal.	8 Ought-feeling.
9 Æsthetic Objects.	9 Hyper-logical.	9 Recog. of Universal Pract. Theoret.	9 Semblance of Intuition (Art).

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(...ession of Psychic Objects.)

...ble I.

Mixed (psychic-psycholog.) Progressions.

Modes.

v. *Interest.*	vi. *Attention.*	vii. *Control (psychic).*	viia. *Control (psychological).*
1 A-telic	1 Att = A	1 Involuntary. Sense Coef.	1 External. Heteronomic.
2	2	2 Unvoluntary. Mem. Coef.	2 Organic.
		Novel.	Fortuitous.
3	3	3 Anomic. (suspended coefficients.)	3
4 Contemplative (Autotelic).	4 Att = $A + \overrightarrow{a}$	4 Voluntary (experimental).	4 Psychological (*e. g.*, association).
5 Practical ($\rightarrow$) (? telic)	5	5 'Prac. Judg.'	5 Real Kinds. Fact
6	6 Att=$A+\overrightarrow{\begin{cases} a \\ a' \\ a'' \end{cases}}$	6 Belief coef. Means to ends.	6 Real Worlds.
7	7 either $\rightarrow$ Att=$A+a+a$ or $\leftarrow$	7 'Workableness.' Laws of Thought.	7 Social Tests. Logical Criteria. Truth
8 Theoretical ($\leftarrow$) (?telic)	8 both $\rightarrow$ Att=$A+a+a$ and $\leftarrow$	8 Moral Coef. Duty. Moral Ideal (self).	8 Right.
9 Contemplative. (Pantelic)	9 Alternative.	Syn-nomic. 9 Æsthetic Coefficient.	9 Beauty.

'social-personal' distinction. This means, it would seem, that a strain of social worth appears in all determinations of judgment. Further, as to judgment, *it* is found to be the criterion of the Logical, properly speaking (col. ii.), although the progressions up to it, through the preliminary dualisms (3, 5, 6), illustrate strikingly the fact of continuity. The modes 3 to 6 are in a very real sense 'quasi-logical.'[1]

Note also that 'Moral objects' (8) are 'extra-logical,' except when made matter of theoretical interest (col. v.), and that 'Æsthetic objects' (9) are hyper-logical, in the sense of having both practical and theoretical Individuation (col. iii.), and also as involving a higher form of interest and control.

Indeed, still speaking of the æsthetic, I may add that another of the points most in need of clearing up, and hence earnestly worked at here, is the relation of the two forms of 'Semblant' objects, those of Play and Art (col. iv., 4 and 9), considered in respect to their psychic meanings, to the other forms of objective construction. It is my conviction that in both of these — and it is part of the fact that *any possible psychic object may be determined as one of them* — we have the genetic resolution of the dualisms and pluralisms of the various cognitive modes as such. So I find it necessary to use terms which lack the partial connotations of those employed for such modes. The æsthetic is 'hyper-logical'[2] (as explained above); it is 'contemplative,' or as regards its end, 'pan-telic,' having both practical and theoretical interest; and it has the further extraordinary character that it is under what I venture to describe as 'syn-nomic' control: that is, it is a form of determination in which both the psychic and also the psychically-recognized-as-foreign conditions of determination are satisfied. There is here a higher psychic immediacy in which all the dualisms of the mental life, at the stage reached, may on occasion merge in an immediate contemplative value of real presence; the dualisms of 'theoretical and practical,' 'mind and body,' 'inner and outer,' 'freedom and necessity,' all merge to the vanishing point in the æsthetic.

It only remains to be added, in the consideration of the objective progressions as such, that the dualism of Self-notself is described as one of 'Content,' inasmuch as it arises only when the dualism of Mind-body, gives place to that distinction within the psychic sphere in which

[1] The progressions in the development of the Logical mode itself are matter for later statement, as are also those of the Self-social mode (cf. this REVIEW, May, 1903, pp. 226 ff.).

[2] Meta-logical, suggested to me and otherwise apt, has been preëmpted for the sense given to it by Schopenhauer (*Fourfold Root*, § 33).

part of the content is set off as 'self' over against the rest of the objective content or 'not-self.'

The progressions of the 'Individuation' mode (iii.) are at once most difficult to trace and most important in relation to the questions of logical value (ii.) and real reference (iv.). The considerations involved are so detailed that the catchwords given in the column iii. must suffice in this place. Yet attention may be drawn to the position that the recognition of 'class' is the term of transition from the Play mode to the Substantive mode, and that the recognition of 'general meaning or intent' is preliminary to the 'general concept' which alone is logical. Psychically there is reason also for maintaining that all individuation is a function of recognition.

The Reality progressions (iv.) are in familiar terms; though I may remark that by 'practical judgment,' I mean the sort of practical use of means to ends recently described by Hobhouse (*Mind in Evolution*) as probably occurring sometimes among animals. Genetically it seems to be closely associated — as regards the psychic elements involved — with the experimental treatment of objects so conspicuous in the Play mode (see col. vii., 4).

In the Interest progressions (col. v.), one should note the overlapping of the practical and theoretical interests, and the arrangement of the *psychic* stages with reference to the ends of the interests, namely, as 'a-telic,' (*e. g.*, without psychic end), 'auto-telic,'[1] practical (*?*-telic),[2] theoretical (*?*-telic),[2] and 'pan-telic' (inclusive of all sorts of ends). It may also be noted that the rise of theoretical interest is put in the transition from the Substantive to the Content mode — the interest which motives the distinction between self and not-self being both practical and theoretical.[3]

The progressions in the Attention mode are sufficiently explained by the explanations of the genetic formula for the attention given in the chapter of an earlier work where that formula is proposed (*Ment.*

[1] Cf. the *Dict. of Philos.*, sub verbo.

[2] Suggestions of proper compounds in these two cases are in order; possibly 'pragma-telic' and 'noö-telic' would do. It is advisable to confine the term 'practical' to the objective psychological point of view, and to use 'pragmatelic' for that psychic; for pragmatelic interest is not at all coëxtensive with practical interest. To make the same distinction general, as between 'telic' (psychic) and 'teleological' function, would aid in banishing the utter confusion which prevails in the use of the latter term. The teleological is the *end-attaining*, to an observer; the 'telic' is the *end-seeking* in psychic process.

[3] Theoretical in the germinal sense of being experimental — a necessary phase of theoretical interest, as I believe.

Devel., chap. X., § 3).[1] The arrows are explained by the terms theoretical and practical with which they are associated in col. v.

The Control modes are necessarily described as 'mixed' in respect to the contrast of psychic and psychological (or objective), inasmuch as the only possible variations in the description of the sorts of control are those characteristic of the two contrasted points of view. For instance, control is 'heteronomic' to the psychic, when it is described as external (*e. g.*, biological, organic, etc.); it is 'a-nomic' when it is or appears to be lawless from both points of view. Later on in the progressions we have terms in use for both sorts of control: 'belief' over against 'truth' (and 'fact'), 'duty' over against 'right,' 'æsthetic quality' over against 'beauty' (this last being a joint sort of control covered by the term 'syn-nomic'). A further point of interest to the writer is that suggested by the double brackets of different lengths between 5 and 7 (col. vii.); namely, the point that the theoretical form of control ('fact' and 'truth') extends from 5 to 7 — over a certain range of objects — while there are also other control-forms extending not only over the same range, but beyond it in both directions.

It may well be that such a schematic presentation as this has no value or suggestiveness; and I should not be surprised to hear this opinion expressed. But the tentative character of the results, and the absence of the detailed grounds which are to my mind reasonably strong, may be just the needed stimulus to some one to treat the topic more fruitfully.

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NOTES.

I trust that the paper of Professor Max Meyer, in the Review for March, on 'Attributes of Sensation,' may stimulate reflection, if not discussion, on that subject. Personally, I prefer the term 'element' for what Dr. Meyer calls 'attribute,' but that is mainly a verbal difference between us. The positive contribution of the paper seems to me to be the teaching that the existence of a psychologically simple 'tone-quality,' which varies with the pitch and yet is introspectively distinct from it, discredits independent variability as a principle of distinguishing 'attributes' of sensation.

[1] The genetic formula $Attention = A + a + \alpha$, in which A stands for the gross muscular and other sensational processes of attention, a the added contractions, etc., of recognizing a class (*e. g.*, visual objects), and α the finer adjustments of individual recognition.

The paper is marred by the uncritical adoption of duration as attribute of sensation in the sense in which qualities, intensities, and extensities are said to be attributes. But duration as content of consciousness is a complex, not an elemental, experience; and duration viewed as attribute is predicated of all events, physical as well as psychical, and is therefore not an attribute at all, in the psychological sense. Even a psychologist who does not admit the preceding statements, ought at least to recognize that the traditional treatment of duration has been challenged.[1]

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On account of the accumulation of material the REVIEW will issue a double number (July–September) on July 1. The PSYCHOLOGICAL BULLETIN of June 15 will also be a double number (June–July) devoted to Mental Pathology, of which Dr. A. Meyer, of the New York Pathological Institute, will be the 'effective editor.' It may be added also that no less than four Monograph Supplements are in our hands for immediate publication. The annual bibliography, THE PSYCHOLOGICAL INDEX, is to appear in a few days. It shows a marked falling off from the last year in the number of titles listed.

THE EDITORS.

[1] Cf. on the duration problem, a paper by Professor M. F. Washburn, this REVIEW, July, 1903, and a shorter discussion by the writer, *ibid.*, vol. VI., 1899, p. 506.